THURSDAY, FEBRUARY 23, 1888.

PHYSICAL SCIENCE AND THE WOOLWICH EXAMINATIONS.

SINCE the appearance of our article of January 26 considerable interest has been manifested in this question, and during the past week an important letter on the subject from the head master of Clifton has appeared in the *Times*. We learn from Mr. Wilson's letter that the new regulations are not only calculated to do harm by the discouragement of science, but that they are also retrograde in another very important particular. By increasing the range of the obligatory examination in mathematics they will not greatly affect the selection of candidates, but, in the case of very many of them, by compelling wider and less thorough study, they will damage the training in that subject. In mathematics the regulations will be likely to stimulate cram.

In answer to a question put by Mr. Howorth, on Tuesday, the 14th inst., the Secretary of State for War is reported to have said that the new regulations are to be of permanent application, and that they are intended (1) to discourage subjects that can be crammed; (2) to give a preponderance to those subjects which are to the majority of officers of greatest practical importance.

Are the regulations calculated to achieve these purposes? We think it can be shown very clearly that they are not. It is therefore with renewed hope that discussion will lead to their amendment that we enter upon the following examination of them.

(1) The new regulations are intended to discourage subjects that can be crammed. Mr. Wilson, as we have already pointed out, has indicated that in the case of mathematics they will distinctly tend to encourage quantity at the expense of quality. With regard to science subjects, the examination statistics which we published in 1884 clearly proved that experimental science had not been chosen by candidates on account of susceptibility to cram. During the three or four years that preceded 1884, a branch of experimental science was offered by only 22 per cent. of the successful candidates; since that date the numbers have risen, notwithstanding the increased thoroughness of some parts of the examination; and in 1887 about 38 per cent. of the successful candidates offered a branch of experimental science. This development is noteworthy, and may be considered to indicate an increased appreciation of the value of such work by teachers and students, since it has taken place in spite of the subject being rather a bad than a good one from the mark-winning point of view, and also during a period notable for improvement in some parts of the examination. Geography and geology, which may be, as some hold, more susceptible of cramming than chemistry and physics, show no corresponding tendency. In 1887 this subject was taken up by a distinctly smaller proportion of successful candidates than in the years that preceded 1884.

There does not appear, then, to be any justification for treating science as a subject more easy to cram than others that are more favourably treated. Had it been true that it is so, it would have been selected by a greater proportion of candidates formerly, and it would certainly have been discouraged by the nature of the examination during the last few years.

(2) Are the subjects selected those most calculated to be of practical importance to a majority of officers in the Engineers and Artillery? A flood of light is thrown upon this point by the course of instruction given to the cadets in the Royal Military Academy.

During the first year of training at Woolwich, cadets study in the compulsory courses the following subjects:—

Mathematics,	for which	3000	marks are given.
Field Fortification,	,,	2000	,, ,,
Military Topography,	,,	2000	,, ,,
French or German,	,,	1000	,, ,,
Chemistry and Physics,	,,	1000	,, ,,
Model Drawing,	,,	300	,, ,,

During the second year of training, the cadets are divided into two classes. Those who are selected for the Engineers then study, in addition to purely professional subjects—

Mathematics,	for which	2000	marks are given.
Chemistry and Physics,	,,	1000	,, ,,
Freehand Drawing,	,,	1000	,, ,,

In the case of the Artillery cadets during their second year, chemistry and physics alone of the ten or eleven subjects examined in the entrance competition are considered to be of sufficient practical importance to be retained.

Thus it stands admitted by the military authorities, according to their own regulations for the education of cadets, that experimental science stands next to mathematics as a subject of practical importance in the training of officers for the scientific branches of the army.

That very great weight should be given to mathematics and modern languages in the examinations for Woolwich cadetships is obviously proper; but since it is admitted, by the courses of instruction in the Royal Military Academy, that capacity for and extensive training in experimental science form part of the necessary equipment of an officer of Engineers or Artillery, a system of selection which includes no means whatever of securing youths capable of such studies in the preliminary examination, and which places youths of scientific powers at such considerable disadvantages, plainly needs to be amended, in the interest both of the service and of the candidates. We do not doubt that those who get into the Academy are excellently taught there, but under these regulations many will be rejected who are eminently fitted to do well, in favour of others who are less gifted with the qualities that are most valuable.

The Committee who framed these regulations have, we fear, forgotten that they will not create a capacity for science work by the mere teaching of science to the successful cadets; the utmost they can do in the case of those whose talents are linguistic rather than scientific,[1] will be to compel them to acquire by hard, uncongenial labour the necessary minimum of knowledge that is required in the subsequent examination. It is abundantly evident that the War Office Committee, probably from inexperience in matters of education, have made a serious mistake. The new regulations, so far as experimental

[1] The enormous value to be given to modern languages must result in many such winning admission to Woolwich.

science is concerned, are needless as precautions against cramming; they will not give due weight to the subjects which are, by their own showing, of most practical importance to officers; and they will influence most unfairly the selection of candidates by giving no chance for scientific power to tell in the results of the examinations.

There is another side of the question which is of very great public importance, viz. the influence of these and other examinations on school work in general. Regulations such as those now in force at Sandhurst, and those about to come into operation at Woolwich, make it increasingly difficult for science subjects to maintain their proper place in schools where they are already adopted, and hinder their adoption elsewhere. Many of the ablest youths in our public schools enter as competitors in these and other examinations, and as they must offer the subjects that pay best, such regulations as those under discussion lower the general standard of school work by constantly withdrawing from the science classes a large proportion of the best students. At present good work in science pays less well very often than mediocrity in other subjects. This, as was pointed out by Sir Lyon Playfair in his Presidential Address to the British Association in 1885, helps to arrest progress in science teaching. We do not, of course, claim that the interests of science in schools should be allowed to outweigh the manifest needs of the public services. But the fact that these examinations do exercise a potent influence, not only on the education of the candidates, but also on the general tendency of school education, throws great responsibility on those who control them, and makes it our duty to urge that this influence shall not be lost sight of, especially in such a case as that of the selection of Woolwich cadets. An aptitude for experimental science in the case of a Woolwich cadet cannot but be an unmixed gain when accompanied by the sufficient degree of mathematical ability that the scheme of examination sufficiently secures.

THE MOTHS OF INDIA.

A Catalogue of the Moths of India. Compiled by E. C. Cotes, First Assistant to the Superintendent, Indian Museum, and Colonel C. Swinhoe, F.L.S., F.Z.S., &c. Part I. Sphinges. Part II. Bombyces. (Calcutta: Printed by the Superintendent of Government Printing, 1887.)

IT is not too much to say that the task of writing a catalogue of the moths of India is one which might appal an entomologist of far longer experience than either of the authors of this work. For when we consider that no general catalogue or revision of the Heterocera exists more recent than that of Guénée, that almost the whole of the types of the described species are in England, whilst both the authors of this book are in India, and that the number of Indian moths is so great that in the two first families alone upwards of 1600 species or supposed species are catalogued, it is evident that the difficulty of such a work is enormous; and as the authors are not known as lepidopterists of long standing, and are resident on opposite sides of India, no one would expect too much from the first attempt at what has long been very much wanted—namely, some work which would enable the rapidly-growing circle of working naturalists in India to know what has already been described and where the descriptions have appeared.

I think, therefore, that the cordial thanks of all will be given to Colonel Swinhoe and Mr. Cotes for their bold attempt to fill this blank, and that no one will be too critical as to how their task has been done when the great difficulties under which they labour are remembered. There is not a word of introduction to say to what extent either of the authors is responsible for the work, but I believe that Mr. Cotes is really the compiler, and that Colonel Swinhoe, whose collection is much richer than that of the Indian Museum in the species which occur in Western India, has added such additional species and notes as he possesses.

The plan of the work is nothing more than a bare catalogue of names and references, with localities so far as known to the compilers or to the authors of these names; and, as we see that in some genera almost all the species are unknown to either of the authors except from the descriptions or plates, it is evident that a large proportion of the names are names and nothing more.

In the genus Syntomis, for instance, we find forty-two supposed species catalogued, of which fifteen are described by Moore, nine by Walker, and ten by Butler; of all these only eleven are in Colonel Swinhoe's collection, and thirteen in that of the Indian Museum, and we do not find that a single attempt has been made to discover how many of these forty-two names represent distinct species.

As long as authors continue to do as Messrs. Moore, Butler, and the late Mr. Walker have so freely done—namely, to describe anything they do not personally know, with little regard to what has already been described—it is evident that, when their views as to variation are also extremely narrow, a great many synonyms must result, and we think a little genuine work would tend to show that of the forty-two supposed species of Syntomis not more than perhaps twenty really exist in nature. It is, however, quite as probable that while not more than twenty distinct species are described from India, at least twenty more remain undiscovered, for it is hardly possible for anyone who does not know India personally to understand how infinitesimal our knowledge of the moths is, except in some half-dozen localities like Bombay, Calcutta, and Sikkim; and even in such places as these what we know is but little compared to what we do not know. Surely here is a field for study and amusement which must attract many who will, sooner or later, provide the materials and collect the knowledge necessary for a "Catalogue *raisonné*," but the sooner a good example is set, by the careful and scientific description of the genera and species which are known, with due regard to distribution and variation, the more and better will be the work done.

A book is projected by Mr. F. Moore, whose knowledge of Indian moths is certainly greater than that of all other entomologists combined; but it is sincerely to be hoped that he will not adopt such a plan or style of work as his recently published "Lepidoptera of Ceylon." The bulk and cost of such a work on the Lepidoptera of India would quite prevent its use by those most likely to use it to advantage, and even if it was completed in

h's lifetime the earlier parts would be out of date before the last were published.

What is wanted is something like Stainton's "Manual of British Lepidoptera," together with a serial publication which would give such a medium to entomologists for publishing their discoveries as *Stray Feathers* gave to Indian ornithologists. When such a journal has been going on for twenty years or so, it will be time to think of a Catalogue of the moths of India really worthy of the name. At present such an ambitious scheme as that proposed by Mr. Moore seems to me only likely to stand in the way of something better hereafter.

It is a great pity that no references are given in this Catalogue to the descriptions of the very numerous genera, so many of which are the creation of Mr. Moore. How many of them will eventually stand, time alone can show, but certainly many of them will merge in genera known in other parts of the world besides India.

I think also that if the authorities for the localities given for the various species were stated, as has been done in the case of specimens in the Calcutta Museum, this would be a great addition to the Catalogue. It is quite as important to know who collected a particular species as to know in whose collection it exists; and many localities are given without any good authority.

Another improvement in the form of the work would be an abbreviation of the references, in the same way as is done in Standinger's "Catalogue of European Lepidoptera"; a short bibliography of works cited, and their abbreviated citations, will take away any possibility of doubt, and save innumerable repetitions of such references as

"Walker, Cat. Lep. Het. B. M.,"
"Moore, Proc. Zool. Soc. Lond.,"
"Felder, Reise Novara Lep.,"
"Butler, Ill. Typ. Lep. Het. B. M ,"

which might be reduced with advantage to

"Walk., B.M.,"
"Moore, P.Z.S.,"
"Feld., Nov.,"
"Butl., B.M."

But notwithstanding the imperfections of this Catalogue, its publication will be a real blessing to naturalists, if only by saving them an immense deal of the most tedious, troublesome, and unsatisfactory work—the hunting up of descriptions and references. How far these are correct, I have not as yet been able to prove; but the few omissions which I have discovered may easily be forgiven. H. J. ELWES.

PROLEGOMENA TO THE STATISTICS OF THOUGHT.

Die Welt in ihren Spiegelungen unter dem Wandel des Völkergedankens. Prolegomena zu einer Gedankenstatistik. ("The Universe as reflected in the Movement of Thought among the Races of Mankind. Prolegomena to the Statistics of Thought.") By A. Bastian. One vol. in 8vo, with an Atlas or Ethnographical Picture-book in oblong folio. (Berlin: E.S.Mittler, 1887.)

DR. BASTIAN'S idea is that the new science of ethnology supplies materials from which it is possible to construct a system of psychology on the inductive methods of natural science. The inductive study of the material universe has given us our modern science, and with it modern materialism. But materialism, says Dr. Bastian, is but a one-sided expression of the legitimate tendency of the age towards induction and natural science. It overlooks the fact that the world of ideas offers as legitimate a field for the application of scientific method as the world of material phenomena. Ethnology, which considers man not as an individual, but in his social aspects, teaches us that the universe of thought also obeys laws, and can be studied by the genetic method. And therefore Dr. Bastian desires to see the statistics of thought put together in a way that will exhibit the whole range of ideas about the universe and its contents which have been prevalent among the various races of men. These statistics will form the basis for a psychology constructed on inductive principles.

The description of a science which has still to be created must necessarily be vague and hazy, and in the present case the vagueness is increased by the fact that Dr. Bastian writes in a very involved and enigmatic style, so that his meaning cannot be read, but must be divined. But so far as we have been able to follow him we gather that in his "Prolegomena to the Statistics of Thought" he designs something different from what is given in ordinary Prolegomena, and that the volume should rather be called a provisional collection of materials for the comparative study of the ideas entertained by different races, or in different stages of culture, as to the universe and the leading matters of human interest that it contains. It would seem that Dr. Bastian, whose great range of knowlege in matters ethnological is well known, and who is also a voracious and somewhat undiscriminating reader of books on all possible subjects connected with the history of human thought, has accumulated huge commonplace-books to illustrate his favourite project. The small-type sections which make up a large part of the volume are simply chunks from these note-books, to all appearance entirely undigested. Commonplace-books have always a tendency to become chaotic, especially in the hands of a man who reads so widely and miscellaneously as Dr. Bastian has done; but we have never seen anything quite so formless as these pages. In themselves many of the extracts given are interesting or curious, but the disorder in which they stand is simply bewildering. Moreover, there are no exact references to chapter and verse of the authors quoted, and verbal excerpts stand side by side with brief jottings and condensed indications such as a man may make for his own use, but which are so many enigmas to the reader. There has evidently been no verification and no revision of the notes originally made by the author for himself, and many of them, therefore, are not only obscure, but not quite accurate; while others were not worth printing at all. The last remark is specially applicable to a vast number of quotations from ancient and modern metaphysicians, into whom Dr. Bastian has evidently dipped at hazard, without having any clear conception of the history of philosophy as a whole. On half a page we find Proclus, Anaximander, Philolaus, Aristotle, the Pythagoreans, F. A. Müller, Spencer, Schelling, Samuel ben Gebirol, and Anaxagoras. Who can hope to be instructed by such a

jumble? The large-type sections that connect these masses of confused notes are still more perplexing. Here, also, the commonplace-book predominates, but the extracts are worked up into some semblance of a continuous exposition. It is very seldom, however, that one can read a page on end without losing the thread. The reason soon becomes obvious. What is offered as a book is really nothing more than a transcript of rough jottings, in which Dr. Bastian had from time to time recorded his ideas in a form just sufficient to preserve a record for his own use. The sentences are often not even grammatical, and in brief the volume is only the roughest of rough note-books printed without revision. In spite, therefore, of the enormous labour and learning which it attests, the whole must be pronounced a failure, for the elementary reason that it is not a book. We trust that the publication may be useful to the author in helping him to get his superabundant material better under control, and so to produce hereafter something that is a book and can be read.

The ethnological picture-book is designed for young people, and its illustrations of cosmogonic and cosmographic ideas, of various conceptions of the future life and so forth, are well calculated to excite their curiosity and stimulate their interest in such things.

OUR BOOK SHELF.

Experimental Chemistry for Junior Students. By J. Emerson Reynolds, M.D., F.R.S. Part IV. Organic Chemistry. (London: Longmans, Green, and Co., 1887.)

THIS volume on organic completes the author's course of experimental chemistry. Whatever may be the opinions on the three previous volumes, there is no doubt this is the most rational attempt to treat organic chemistry practically—as a thing for students actually to do—that has as yet appeared. There is scarcely an experiment in the book that a student will be unable to do from the description given, and the order in which they are taken and general arrangement is the natural order of synthesis, proceeding from the less complex and easy to the most complex and least known.

The author begins with destructive distillation, and the production of alcohols, their salts, &c. The fourth chapter deals with metallic compounds or organo-metallic bodies. In the description of the manufacture of zinc ethide the method of making from zinc, C_2H_5I, and iodine might have been given, as the action is much quicker than with the Cu—Zn couple and the yield greater. The current of CO_2 can also be dispensed with advantageously. Two experiments here we must take exception to as being rather dangerous for beginners—sealing up sodium with zinc ethide, and in Experiment 691 making mercuric ethide as a sort of starting-point material. The author cautions against inhaling the vapour of this substance, as it is "supposed to be poisonous." We thought it was quite settled that it is about the most dangerous substance one has to deal with; and we certainly do not agree with the author that the method of employing mercuric ethide for making zinc ethide is the easiest of all methods for making the last-named substance.

In the remainder of the little book there is nothing either in arrangement or process to which objection can be taken, and undoubtedly it should be most useful to students attending a course of organic lectures. As a rule English students stop off with organic before they have really made its acquaintance; very few indeed continue its study long enough for it to be of any use to them. Most of the works in England where "organic chemistry" is the rule are obliged to obtain the services of German chemists; the English student's acquaintance with the subject generally stopping at the knowledge that there are such things as hydrocarbons, or "hydrocarbides," as the author of this book calls them.

Perhaps when such practical instruction is given in our schools as the course outlined by this book, we may begin to produce students who can go into "a works" and be trusted not only to follow a process but to originate new ones. W. R. H.

The Farmer's Friends and Foes. By Theodore Wood, F.E.S. (London: Swan Sonnenschein, Lowrey, and Co., 1888.)

THIS is a well-meant and well-put-together little volume, giving an account of the life-history of most of the animals which, for good or for evil, come across the path of the British agriculturist. Throughout, the attempt is made to prove that, when it is necessary for the saving of a crop to destroy any animal, it is far better to trust to Nature, as being more competent, than to man; but then this seems to beg the whole question, as it presumes that man has not already very much interfered with Nature's regulations.

The volume is, in part, the result of personal investigation, but the author quotes freely from all our best-known writers on the subjects of which he treats.

The figures are good. A table of contents would have added to the usefulness of the work, especially as the index is not very detailed. The volume may be safely placed in the hands of all interested in the subject.

The Story of Creation. By Edward Clodd. (London: Longmans, Green, and Co., 1888.)

THE author of this book does not pretend to make his readers acquainted with new facts and ideas. His object is to present a popular exposition of the theory of evolution, using the word evolution in its widest sense. The work is divided into two parts—one descriptive, the other explanatory. In the descriptive part he begins with a chapter on matter and power. He then considers the distribution of matter in space, and gives a general account of the sun and the planets, of the past life-history of the earth, and of present life-forms. In the explanatory part he discusses the questions relating to inorganic evolution and to the origin of life and life-forms, and sets forth in logical order the arguments which are held to establish the truth of Darwin's theory of the origin and development of species. A final chapter is devoted to social evolution, including the evolution of mind, society, language, art and science, morals, and theology. The book is vigorously written, and well illustrated; and readers who have had no special scientific training will find that it enables them to understand and appreciate some of the greatest and most fruitful generalizations of modern science.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Botanists and the Micromillimetre.

I NOTICE that in a review of a "Manual of British Discomycetes" which appeared in NATURE on February 9 (p. 340), and apparently also in that work itself, the word *micromillimetre* is used as equivalent to the thousandth of a millimetre.

I have made some inquiry, and am told that it is now commonly employed by biologists, and especially by botanists, with that meaning.

As it would be very unfortunate if the same word were habitually used in different senses by students of different branches of science, may I be allowed to point out that, according to the definitions of the C.G.S. system, a micromillimetre is the millionth of a millimetre.

In the well-known Report of the Committee of the British Association for the "Selection and Nomenclature of Dynamical and Electrical Units" it is laid down that the prefixes *mega* and *micro* are to be employed "for multiplication and division by a million."

This ruling has been generally accepted not only by scientific men, but also by those engaged in commerce. Megohm and microfarad are terms which are used in contracts, and are universally understood to mean a million ohms and a millionth of a farad respectively. It will be hopeless to try to introduce scientific systems of measurement into the affairs of daily life if scientific men themselves disregard the rules on which those systems are framed.

It would also be particularly confusing if the micromillimetre were wrongly used by microscopists. In its proper sense it is the most convenient unit in which to express molecular magnitudes. It has been employed for that purpose by Sir William Thomson and others in England, and also by physicists abroad. If the micromillimetre of the microscopist is 1000 times too large, all sorts of mistakes will be rife as to the relative dimensions of molecules and of the smallest objects visible with the microscope.

The proper name for the thousandth of a millimetre (μ) is the *micromètre*, and though the similarity of this word to *micromèter* is no doubt a drawback, it is not likely that confusion could often arise between them.

If, therefore, I am rightly informed as to the custom of botanists in this matter, I beg respectfully to suggest that they should bring their nomenclature of units of length into conformity with the definitions of the C.G.S. system. Otherwise there will be a permanent confusion between the *micrometre* (μ) and the micromillimetre ($\mu\mu$). ARTHUR W. RÜCKER.

Science Schools, South Kensington, February 17.

"The Teaching of Elementary Chemistry."

ALLOW me to draw the attention of the chemical section of your readers to a few highly misleading passages in the two books reviewed under the above heading in NATURE of January 19 (p. 265).

On p. 65 of the "Elementary Chemistry" we read :—

"Hence when sodium and water interact, a portion of the hydrogen which was combined with oxygen is evolved as hydrogen gas, and another portion enters into combination with the sodium and the oxygen to produce caustic soda."

On pp. 116-17 is to be found the following astounding passage :—

"To prepare Cl_2O, mercuric oxide (HgO) is heated in a stream of dry chlorine. When mercuric oxide is heated it is decomposed into mercury and oxygen; therefore by passing chlorine over heated mercuric oxide we carry out a reaction in which oxygen is produced in presence of chlorine."

In the "Practical Chemistry," under Experiment 1, Chapter II. (p. 6), occurs the following warning to the student :—

"Do not remove the lid at any time for more than a second or so, else some of the magnesia will be volatilized and lost;" while on p. 285 of the "Elementary Chemistry" we read :—

"No compound of Mg has been gasified."

Even this contradiction is excelled by one on pp. 62 and 63 of the "Practical Chemistry," which is not so manifestly a slip. We read (p. 62) :—

"The reactions between aqueous solutions of alkalies and the three elements, chlorine, bromine, and iodine, are similar; compounds of similar compositions and similar properties are produced under similar conditions."

Under Experiments 19 and 20, which follow, the student is told to treat cold solutions of potassium hydroxide with chlorine and bromine respectively. The well-known changes are described, and the bleaching properties of the solutions after addition of a little acid, are to be tried. Then follows (p. 63) :—

"Exp. 21. Perform an experiment similar to 19 and 20, but use *iodine* in place of chlorine or bromine: the liquid which is produced does not bleach. No compound of iodine analogous to $KClO$ and $KBrO$ has been obtained."

Truly this is "seeing things as they are" with a vengeance.

Z.

Natural Science and the Woolwich Examinations.

MAY I be allowed, as one of the most experienced science masters in the public schools, to say a word in reply to Mr. Gurney's letter in NATURE of this week (February 16, p. 365)?

There seems to me a general fallacy running through that letter arising from "the absence (on the writer's part) of clear discrimination" between the nature and methods of mathematical science (which, as J. S. Mill taught us long ago, are mainly *deductive*) and experimental science (which proceeds by *inductive* methods). It is on this ground mainly, coupled with the extent to which it cultivates the faculty of *observation*, that we claim for it a special educational value.

After an educational experience at least as extensive as Mr. Gurney's, I join issue with him most distinctly on this point. I am afraid there lurks behind Mr. Gurney's depreciation of the educational value of science the disappointment which other mathematicians have experienced in finding that the man who takes to experimental science as a mere excursion-ground for the diversion of the mathematician is not infrequently brought to confusion by Nature. *Science is something more than measurement.* Mr. Gurney's notion that mathematics and a knowledge of French and German are a sufficient groundwork for true scientific knowledge is such a confession on his part of the small value he attaches to experimental demonstration and to laboratory training (or to field-work in the case of geology) as is sufficient to put him out of court as a witness on this question. Nor do I think that he is competent to speak with any authority on the work done in the public schools. If he fancies that the best boys of the public schools go to Powis Square to finish their education, he is labouring under a delusion.

The whole argument of the letter is retrogressive; nor is it strengthened by the writer's condemnation of a "smattering" of science, which is no discovery. But I maintain that a boy can, by the age of eighteen, get a sound groundwork in science laid, though not by the cramming system; and that to this the term does not apply at all. Again, he condemns premature specialization of a boy's studies in favour of science, while he inconsistently advocates a much narrower specialization in favour of mathematical studies.

How far Mr. Gurney is from being convinced by his own arguments is shown by the fact that in the concluding paragraph of his letter he practically surrenders the point on which the whole question turns.

In conclusion I would commend to his careful consideration the letter which appears in the *Times* to-day from the head master of Clifton College, whose competence to form a judgment on the educational aspect of this question I suppose no one doubts. A. IRVING.

Wellington College, Berks, February 17.

WITH your kind permission, and in consideration of the importance of the matter, I crave leave for space for a few remarks in addition to those contained in my reply to Mr. Gurney.

(1) Looking at the history of education in this country, we can account for, though we deplore the existence of, a prevalent notion, a sort of fashionable superstition, which regards scientific studies as outside the range of what is called "culture"; a superstition for which some of those who have spoken in the name of science are not altogether unanswerable, but which derives its chief strength from that profound ignorance of natural science—its nature, its methods, and its object—upon which so large a proportion of educated Englishmen seem rather to pride themselves than otherwise. There can be but little doubt that this has been turned to account as an influence in favour of the contemplated scheme.

(2) It is in no spirit of hostility to literary studies that one writes in these terms; on the contrary, it is as a lover of literature of the better sort that one would gladly see the literary spirit in this country, as in Germany, strengthened and braced by the strong atmosphere of scientific criticism, and a little more first-hand acquaintance with things as they are, which is the true

aim of science. It is impossible to estimate the good that might be done in this direction, if only the Universities had the wisdom and the courage to insist upon a knowledge of some one branch of science for all degrees, as was strenuously advocated years ago by Charles Kingsley.

(3) It seems a great pity that such a change as is contemplated should be adopted just now, since within the last three or four years some of us who are teachers of standing and experience have gladly recognized considerable improvement in the examinations in science as they are conducted by the Civil Service Commissioners. It would appear that the *cramming* of these subjects has been considerably handicapped, if one may judge from the considerable increase in the number of o's affixed to the names of candidates in the published lists *pari passu* with a considerable increase in the number of marks gained by one's own pupils, who have had the same teaching and laboratory training as those of previous years.

(4) It is surely fairly within the province of Parliament to consider the question whether it is expedient or conducive to the common weal, that science shall be placed at such a disadvantage that young men, who are candidates for the more scientific branches of the military service, shall be strongly tempted to eschew all preliminary training in science, as they certainly will be unless the regulations are somewhat modified.

Four years ago the action of a single Member of Parliament (Sir John Lubbock), backed up by the influence of the Councils of the Royal Society and the British Association, was effectual in securing a reconsideration of the provisional examination scheme for admission to Sandhurst; so that, although—as ultimately issued—the regulations contained an absurd anachronism in the proportion of marks assigned to scientific subjects, this was reduced to less startling proportions.

Can it be doubted, then, that if on the present occasion the three Members of Parliament who may be said to be the representatives *par excellence* of science in the Legislature (the President of the Royal Society, the President of the British Association, and the Member for the University of London), were to take united action in Parliament, the position of science (so far as the Army Entrance Examinations are concerned) might be changed from one of semi-strangulation to one of free and fair competition, which is all that its most ardent advocates can desire for it? This could be effected to the advantage of the studies of the cadet, by such a simple modification of the published regulations as would be involved in limiting the choice of optional subjects in Class I. to two, and allowing two subjects of Class II. to be taken up. A. IRVING.

Wellington College, Berks, February 20.

The Composition of Water.

PROF. THORPE, in his interesting article on the composition of water (p. 313), alludes to Dr. Scott's very valuable determinations of the ratio of the volumes of hydrogen and oxygen which combine to form water.

If we assume with Dr. Scott that the small amount of impurity present in his gases, and which he estimated in each case after the explosion, was evenly distributed between them, a curious relation may be observed between the amount of impurity present and the calculated ratio of the volumes.

This relation is apparent if we subtract the ratios calculated by Dr. Scott from some fixed number, say 2·000, and compare these differences with the relative amounts of impurity. It will be seen from the following table that the greater the amount of impurity present the greater is the difference of the ratio from the constant number, or, in other words, the lower is the ratio. The impurity is given in volumes per 100,000, and the differences are multiplied by 10,000.

Relative impurity.	Difference of ratio from constant.	Relative impurity.	Difference of ratio from constant.
35	83	102	125
36	55	105	133
41	66	106	140
47	44	146	154
52	32	162	106
56	90	167	224
66	72	188	118
72	46	254	187
75	80	495	540
78	120	498	506
98	73		

The relation is better seen, however, by mapping the results, taking the ratios as abscissæ, and the impurity in volumes per 100,000 as ordinates. The dotted straight line (Fig. 1) passes so well through the points that it leaves ten of them on the one side and eleven on the other.

It seems difficult to believe that this apparent relation can be merely a chance coincidence; the direction taken by the points is too definite. Nor can it well be due to any chemical action

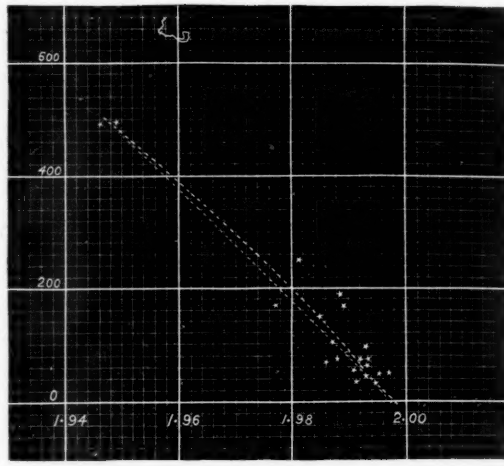


FIG. 1.

between oxygen and nitrogen, for Dr. Scott states that the water produced was free from any acid reaction, and that no trace of the oxides of nitrogen could be detected. The relation is even more marked if we assume that the whole of the impurity was in the hydrogen. This is shown in Fig. 2; the points obviously fall about a line which is nearly, if not quite, straight.

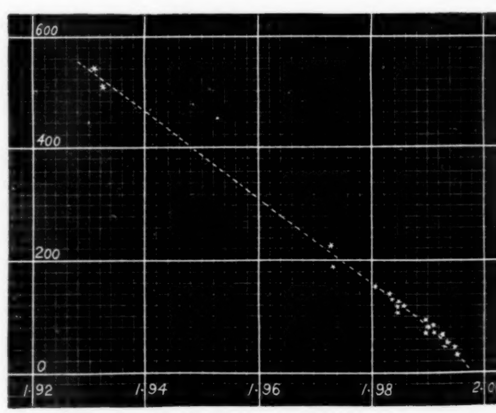


FIG. 2.

If, however, we assume that the whole of the impurity was in the oxygen, and if we neglect the two experiments with the excessive amount of impurity, no such relation is to be observed (see Fig. 3), but the ratios are distributed with fair regularity about a mean value of 1·9965 or 1·9970. The simplest explanation of the facts appears to be that the whole, or at least the greater part, of the impurity was really in the oxygen, and that the apparent relation of the amount of impurity to the ratio

is due to the error introduced into the calculations by referring the impurity to the hydrogen. But, whether this explanation be accepted or not, it is clear that the three lines drawn through the points representing the three series of ratios ought to meet at a point on the horizontal line of zero impurity, for the errors, whether due to chemical action or to calculation, would disappear with the cause that produced them. Hence, if no other source of error is present, the true ratio may be found by taking the most probable point of intersection of the three lines on the horizontal line of zero impurity. It is not easy to determine exactly the position of this point: it probably lies between the values 1·996

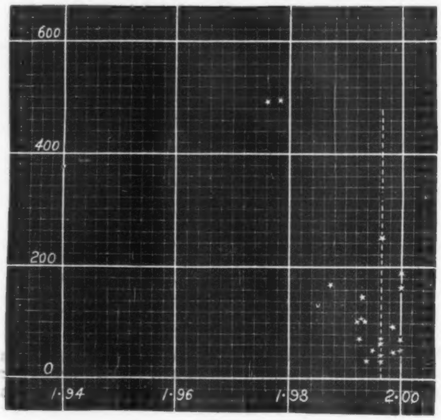


FIG. 3.

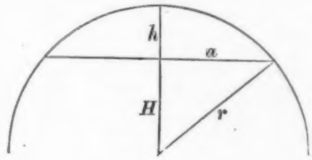
and 1·998, and the true ratio may perhaps be taken as 1·997 Dr. Scott adopts the ratio 1·994, but this appears to me to be certainly a little too low.

Prof. Thorpe shows that the atomic weight of oxygen, calculated from Regnault's densities of oxygen and hydrogen, corrected by Prof. Le Conte, and Dr. Scott's ratio (1·994) for the combining volumes, is 16·019. The ratio 1·997 would make the atomic weight O = 15·985. SYDNEY YOUNG.

University College, Bristol.

The Fog Bow and Ulloa's Ring.

IN the summer of 1875, I made a tour of inspection to our meteorological stations in the surveying-steamer *Hansteen*, Capt. M. Petersen, R.N. During the morning hours of August 7, I was on shore at Gandfjord, on the south side of the Varangerfjord, and measured the height of some terraces there. At 1h. 10m. p.m. we took serial temperatures in the Gandfjord with the deep-sea thermometer. The weather was calm, and a dense fog prevailed. The temperature of the air was 12° C. Leaving the Gandfjord we proceeded northwards. The dense fog continued. At once the fog began to be lighter and the sun to shine through, and a few minutes afterwards we were out of the fog, which was standing as a white wall in the south-west. In the moment the sun appeared, but before we were quite clear of the fog, I saw in the north-east a bow having the shape of a rainbow, but quite white, projected on the fog. With a sextant I measured its amplitude, or the chord along the horizon, and the height of the summit above the horizon—in both cases the middle between the outer and inner edge of the bow. The horizon not being distinctly visible, it is probable that the measures taken do not exactly refer to the true horizon, nor is it certain that the height of the summit was taken from the same horizontal plane in which the amplitude was measured. By the captain's reckoning, the apparent ship's time, at the moment of observation, was 2h. 40m., and the latitude 70° 1'. From these data, and the declination of the sun, I computed the azimuth of the sun at south 46° 5' west, and its apparent altitude at 31° 12'. Supposing, as the results of the several computations tend to indicate, that the white bow is circular, and has its centre in the anthelic point, we may calculate the angular radius of the bow by three different methods.



Let h represent the height of the summit of the bow above the horizon, a half the amplitude or chord along the horizon, H the dip of the centre of the bow below the horizon, supposed to equal the altitude of the sun, and r the angular radius of the bow. Then we have—

$$r = \mathrm{H} + h \quad . \ . \ . \ . \ . \ . \ . \ . \ . \quad (1)$$

$$dr = d\mathrm{H} + dh \quad . \ . \ . \ . \ . \ . \ . \ . \ . \quad (1')$$

$$\cos r = \cos a \cos \mathrm{H} \quad . \ . \ . \ . \ . \ . \ . \ . \quad (2)$$

$$dr = \frac{\tan a}{\tan r}\, da + \frac{\tan \mathrm{H}}{\tan r}\, d\mathrm{H} \quad . \ . \ . \ . \ . \ . \quad (2')$$

$$\cos r = \cos a \cos (r - h)$$

$$\tan r = \frac{1 - \cos a \cos h}{\cos a \sin h}, \text{ or putting}$$

$$\cos a \cos h = \sin^2 \mathrm{M}$$

$$\tan r = \cot^2 \mathrm{M} \cot h \quad . \ . \ . \ . \ . \ . \ . \quad (3)$$

$$dr = \cos (r - h) \frac{\cos r}{\sin h} \tan a\, da - \sin (r - h) \frac{\cos r}{\sin h}\, dh \quad (3')$$

The observations gave $2a = 49°$, $a = 24° 30'$, and $h = 7°$ (or a little more).

From (1) we have $r = 31° 12' + 7° = 38° 12'$.

Putting $d\mathrm{H} = \pm 2'$, $dh = \pm 15'$, we have by (1')

$$dr = \pm \sqrt{2^2 + 15^2} = \pm 15'{\cdot}1 = \pm 0°{\cdot}25.$$

From (2) we have $r = 38° 53'{\cdot}5$,

and by (2') $dr = 0{\cdot}577\, da + 0{\cdot}770\, d\mathrm{H}$, $d\mathrm{H}$ being the error in the altitude of the measured chord, or the chord's altitude or depression, reckoned from the horizon.

Putting $da = d\mathrm{H} = \pm 15' = \pm 0°{\cdot}25$, we get—

$$dr = \pm 0°{\cdot}24.$$

From (3) we have $r = 41° 8'$,

and by (3') $dr = 2{\cdot}315\, da - 3{\cdot}074\, dh$.

Putting $da = dh = \pm 0°{\cdot}25$, we get—

$$dr = \pm 0°{\cdot}97.$$

Taking the weights inversely as the squares of the probable errors, we find that the results from (1) and (2) have a weight of 15 times that found by (3), and the mean will be—

$$r = 38° 38' \pm 6'{\cdot}4.$$

From this mean we find that h should have been 38° 38' − 31° 12', or 7° 26' instead of 7°, or somewhat greater than measured, as supposed in my note-book. Computing from (2) we find that we should have calculated with H = 30° 51' instead of 31° 12', or the chord has been measured in a level 21' lower than the horizon, which is highly probable with the fog spreading over the surface of the sea. The measured chord being too great, and the measured height too small, it follows from (3') that (3) must give the radius by far too large.

The next occasion I had to observe the fog bow was in 1878, on the North Atlantic Expedition, when returning from Spitzbergen. During August 30, our ship, the *Vöringen*, had a rather tedious work in advancing southwards, on account of the foggy weather prevailing the whole day. In the afternoon we had advanced so far south of Bodö as to approach the Sandhorn, a mountain about 3000 feet high, lying to the east of the route.

At 5h. 20m. p.m. I saw an anthelic fog bow, white, with the outer edge reddish, the inner edge bluish. I measured, with the sextant, the amplitude along the horizon at 76°, the sun's altitude at 12°, and the breadth of the bow at 2°. The temperature of the air was about 14° C. The latitude was about 67° 10'.

Assuming the measured chord to lie in the true horizon, we get by (2) from $a = 38°$, H = 12°, $r = 39° 35'{\cdot}5$. But it is highly probable, that the measured chord lay deeper than the

fog-veiled horizon, perhaps some degrees deeper, and it may be quite as possible that I have measured the diameter as a chord. This supposition gives $r = \frac{1}{2} 76°$ or $38°$. The mean of these two determinations is $38° 48'$, with a probable error surely not less than $\pm 48'$, or half the difference.

At 6h. 1m. p.m., the circumstances were more favourable. I measured $2a = 76° 11'$, $H = 8° 11'$, the breadth of the bow 2°. The latitude was 67° 7'. From these data we have, by (2) $r = 38° 50'$. Assuming the observed amplitude to have been the diameter, which is very probable, we have $r = 38° 5'{\cdot}5$. The mean value is $38° 28' \pm 22'$.

If the fog bow, like the rainbow, has always the same diameter, we can join the three values thus found for the radius into a mean result. We have thus, giving the single determinations a weight inversely as the squares of their probable errors—

1875 August 7, 2h. 46m.		$r = 38° 38' \pm 6'$
1878 August 30, 5h. 20m.		$r = 38° 48' \pm 48'$
1878 August 30, 6h. 1m.		$r = 38° 28' \pm 22'$
Mean		$r = 38° 38' \pm 1'{\cdot}4$

The breadth of the bow being 2°, with a probable error of $\frac{1}{20}$ or $\pm 6'$, we get—

for the outermost red ring $r = 39° 38' \pm 6'{\cdot}2$
for the innermost blue ring $r = 37° 38' \pm 6'{\cdot}2$.

At 7h. p.m. the bow stood white against the blue sky, the Sandhorn below it. At 6h. 40m. p.m., and sometimes before, I remarked that my own shadow was visible on the fog wall. In order to get a wider view of the phenomenon, I went up upon the roof of the chart-house, where my eye was 27·5 feet (8·4m.) above the surface of the sea. From here I saw how my shadow distinctly imitated all my movements. The shadow of my head appeared dark on a lighter white ground, and from a certain distance surrounded by a concentric coloured glory, in which the colours were arranged in the order of the spectrum, so that the outermost circumference was red, the middle yellow, and the innermost blue. There was no white band in the glory. With the sextant I measured the radius of the yellow ring, which was the most intense, at 1° 31'. The intensity of the other coloured rings was too feeble to allow their radius to be measured with the sextant. From a comparison with the radius of the yellow ring I judged that of the blue at 1° 15', and that of the red at 1° 45', with a possible error on both sides of $\pm 5'$. This phenomenon is Ulloa's Ring.

Taking all my results together, we have the following synoptical table:—

Ulloa's Ring.	Radius of inner	blue	$1° 15' \pm 5$.
,, ,,	,, ,,	yellow	$1° 31' \pm 2'$.
,, ,,	,, ,,	outer red	$1° 45' \pm 5'$.
Fog Bow	,, ,,	inner blue	$37° 38' \pm 6'$.
,, ,,	,, ,,	middle	$38° 38' \pm 1'$.
,, ,,	,, ,,	outer red	$39° 38' \pm 6'$.

The fog bow cannot be the rainbow with three or four inner reflections, as these rainbows, if visible, would not be anthelic, but have, for the red rays, distances from the sun of 42° and 43°. Moreover, the intensity of the fog bow is too considerable to be the result of so many reflections in drops of rain. The ordinary or first rainbow, with one inner reflection, has a radius or distance from the anthelic point of 42° 30' for the red, and 40° 30' for the violet rays, which gives, the sun's radius being 16', its innermost radius like 40° 14'. The outermost red ring of the fog bow has a radius of 39° 38'. Its distance from the ordinary rainbow is consequently only 36'. This space we see sometimes covered by the supernumerary rainbows, caused, as Sir G. B. Airy's investigations have shown, by the interference of the rays leaving the raindrop.

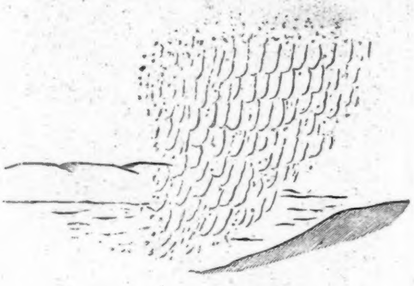
It seems probable that the smallness of the fog globules, as contrasted by the larger size of the raindrops, must enter as an essential part in the explication of the fog bow. In Günther's "Lehrbuch der Geophysik und physikalischen Geographie," ii. p. 151, he speaks of white rainbows, the description of which agrees with the aspect and position of my fog bows, and for which Bravais has given an explanation ("Sur le Phénomène de l'Arc-en-ciel blanc," *Ann. de Chim et Phys.* [3], vol. xxi. p. 348). Not having Bravais' memoir at hand, I may only remark that, as far as I can see from Günther, he assumes the fog drops to be hollow, a supposition which is hardly in accordance with modern investigations.

When I saw the fog bow, I had, I am sorry to say, no polariscope, so that I was unable to investigate the polarization of its light, so characteristic for the rainbow. H. MOHN.

Christiania, January 31.

The Shadow of a Mist.

THE reticulated rippling shadow of the mist described in Mr. Fawcett's letter (NATURE, January 5, p. 224) reminds me of a somewhat parallel phenomenon seen by me a few years ago. I saw a snow-storm some miles away crossing the valley between the Mendips and the Quantocks. It hung like a long, heavy curtain partially obscuring the *bright* western sky. The



light shining through the shower showed a fairly regular pattern. On a reddish-brown ground the darker, because denser, parts of the shower took the form indicated roughly by the accompanying diagram. Was the snow falling in spiral streams, and would a similar explanation apply to the shadow of the mist seen by Mr. Fawcett? HENRY BERNARD.

The English Church, Moscow, January 31.

Instability of Freshly Magnetized Needles.

I MADE no attempt to investigate the fluctuations of the dipping needle. They seemed to me to pass away after a few minutes, and I therefore took that method to get rid of them, supposing that the phenomenon was well known to other observers. The variations that I observed amounted to three or four minutes, I should think. It is true that the dip circle which I used was of an ancient pattern; as Prof. Rücker says, hardly up to modern requirements. I did not send it back to the maker for adjustment, as Mr. Whipple says he would have done, because it was lent to me, and it was the best I was able to get.

Recognizing the fact that we could not expect to get the best results from our outfit, it was deemed best to make only one set of observations at each station, and multiply the number of stations as greatly as possible. This made it necessary to do the work quickly at some stations in order to adapt our time to that of trains, or in order to get the drudgery involved in camp-life done within the twenty-four hours. It is probable that at some stations we overdid the matter, and that the observations would have been better if more time had been taken. The dip observations I always regarded as least satisfactory. But all of the work has been published in such a way that its value can be estimated by anyone interested, and everyone is welcome to place whatever value he pleases upon it. We did the best we could under the circumstances, and the expense was met by private means.

The dip circle was returned to Washington when we were done with it, so that I am not now in a position to throw any light upon the subject under discussion. For most of the stations at which observations were made, I think the magnetic elements were determined with as great precision as a single observation would give them, and they seem to me to be as valuable as they profess to be, and not very much more. The fact that so little magnetic work had been done in the central part of the United States seemed to me to justify the plan of making the number of stations large, rather than of trying to attain the precision of observatory methods in field work at a few stations.

FRANCIS E. NIPHER.

Microsauria and Dendrerpeton.

In the notice in NATURE of January 12 (p. 244) of Fritsch's new number of his excellent work on the Permian fauna of Bohemia, which has not yet reached me, I observe a reference to *Microsauria*, which would seem to imply that I had included *Dendrerpeton* in that group. Possibly this was not intended by the reviewer, as it certainly could not have been intended by Fritsch, who knows my views quite well; but in case it should be misunderstood I beg to say that I have not held this view, but on the contrary have confined the name Microsauria to species with simple teeth, and have placed *Dendrerpeton* with Labyrinthodonts, though by no means as a typical genus of that group. In my last paper on this subject (Trans. Royal Society, 1882), I expressly exclude the two species of *Dendrerpeton* referred to from Microsauria, and define the latter as having non-plicated teeth (pp. 634-35). I may add, however, that I have always held and now hold that the Microsauria, though in some respects inferior to Labyrinthodonts, embrace in their structures premonitions of the true reptiles not found in the latter.

The study of these creatures was one of those bye-efforts thrust on me by circumstances, and which I have closed up so far as I am concerned in the paper referred to; but I have learned to love the little Microsaurs and to regard them as a hopeful and prophetic group. J. WM. DAWSON.

McGill College, January 26.

A New Historic Comet?

PERMIT me to inform Mr. Knott that the "new historic comet" is not a new comet at all. He will find it as No. 154 in Mr. Chambers's Catalogue No. II. in his well-known "Handbook of Descriptive Astronomy." It is there described, 302 A.D. "in May-June a comet was visible in the morning—(Ma-tuoanlin: Williams 26)." W. H. S. MONCK.

Dublin, February 10.

The Proposed Teaching University for London.

NO one, I am sure, who has carefully read my letters in the *Times* on this subject could agree with the writer of your article that I appear "to consider the dispute as one between the efficiency of 'lectures' on the one hand, and of 'reading' on the other."

The writer of the article has certainly misunderstood my views "upon the matters in dispute," as well as my object in quoting Darwin's dictum on the advantages of "reading" as compared with "lectures." PHILIP MAGNUS.

Exhibition Road, London, S.W., February 10.

Institute of Chemistry.

WITH reference to a circular letter dated the 12th inst., and bearing the signature of Mr. W. Thomson, which has been sent to the Fellows of the Institute of Chemistry, we beg that you will be so good as to allow us to inform the Fellows, through your columns, that we have not been consulted in regard to the action taken by Mr. Thomson, and that we decline to offer ourselves as candidates for election in opposition to the nominations of the Council. BOVERTON REDWOOD.

London, February 20. ALFRED GORDON SALAMON.

CORAL FORMATIONS.

I DESIRE to call attention to a condition of reef that I think has been very little studied, but that may contain a clue to a solution of some of the difficulties that still surround the subject of coral formations generally.

I may as well at once avow myself to be one of those who, on reviewing the later evidence on coral growth, have come to the conclusion that it is sufficient to justify an abandonment of the supposition that subsidence plays a principal part in the production of barrier reefs and atolls, but are at the same time not satisfied with one part of the explanation offered by Mr. J. Murray.

I refer to the great effects attributed by him to the disintegration and solution of dead coral by the chemical action of sea-water, in hollowing out and deepening the large and deep lagoons inside both these classes of reefs.

Mr. Murray's theory on this point, as summarized by himself, is that—

"(1) When coral plantations build up on submarine banks, they assume an atoll form, owing to the more abundant supply of food to the outer margins, *and the removal of dead coral rock from the interior portions by currents and by the action of the carbonic acid dissolved in sea-water*. (2) That barrier reefs have built out from the shore on a foundation of volcanic debris, or a talus of coral blocks, coral sediment, or pelagic shells, and *the lagoon channel is formed in the same way as a lagoon*."

The italics are mine, and indicate the part of his theory to which from my view, and, I believe, that of others, there are objections, but to which Mr. Murray attaches considerable weight.

Is it necessary thus to invoke the aid of dissolution of the dead coral by chemical action as an important agent in the formation of these deep lagoons and channels? I think not.

An examination of the forms of, and depths on, well-surveyed submerged banks in different regions reveals a considerable number of reefs, which, if their development continues on the same lines as apparently heretofore, must, in the course of time necessary to bring them to the surface, form perfect atolls of large size, inclosing deep lagoons, without any further scooping out by solution.

Many instances occur in the China Sea. The Tizard Bank, in lat. 10° 20′ N., and long. 114° 25′ E., is 32 nautical miles in length, with an extreme breadth of 10 miles, and was well surveyed in 1867. The central portion is very flat and almost void of patches. Its depth is from 30 to 47 fathoms. Its edge is crowned with a coral rim varying from 4 to 10 fathoms in depth, broken here and there by openings, in some cases over 30 fathoms deep. The bank rises steeply from deep water, but, as no sectional soundings were taken, the precise angle of slope is unknown. The rim is composed of coral in luxuriant growth, and it can scarcely be doubted that in time it will reach the surface. In fact, on its periphery of 100 miles, in eight places small patches of reef, three of which bear islets, have already done so.

When the remaining portions of the rim are also awash, the reef will be in all respects an atoll similar to the great Maldive atolls, without any necessity for solvent action enlarging or deepening it.

Eight other banks of similar character, and in various stages, occur not far from this reef.

The great Macclesfield Bank, farther north, over 70 miles in length, and 40 miles in width, is of precisely the same nature, but its development is not so far advanced; the rim being in no spot nearer the surface than 10 fathoms, the water on it varying from that amount to 19 fathoms, while the depth of the inclosed area is from 40 to 60 fathoms. The survey of this bank is not so complete as in the case of some others, but enough has been done to show its character very plainly.

The Prince Consort Shoal (300 miles S.W. of the Tizard Bank) is apparently at a still earlier stage, a few patches of 17 fathoms and a considerable area of 30 fathoms partly inclosing a central area of 40 fathoms depth. The great Seychelles Bank in the Indian Ocean, 200 miles by 100, is very imperfectly known, but in most places the lines of soundings over its edge exhibit this tendency to form a rim. Here, however, the general depth on the bank is not over 30 fathoms. The Amirante Bank is a similar example.

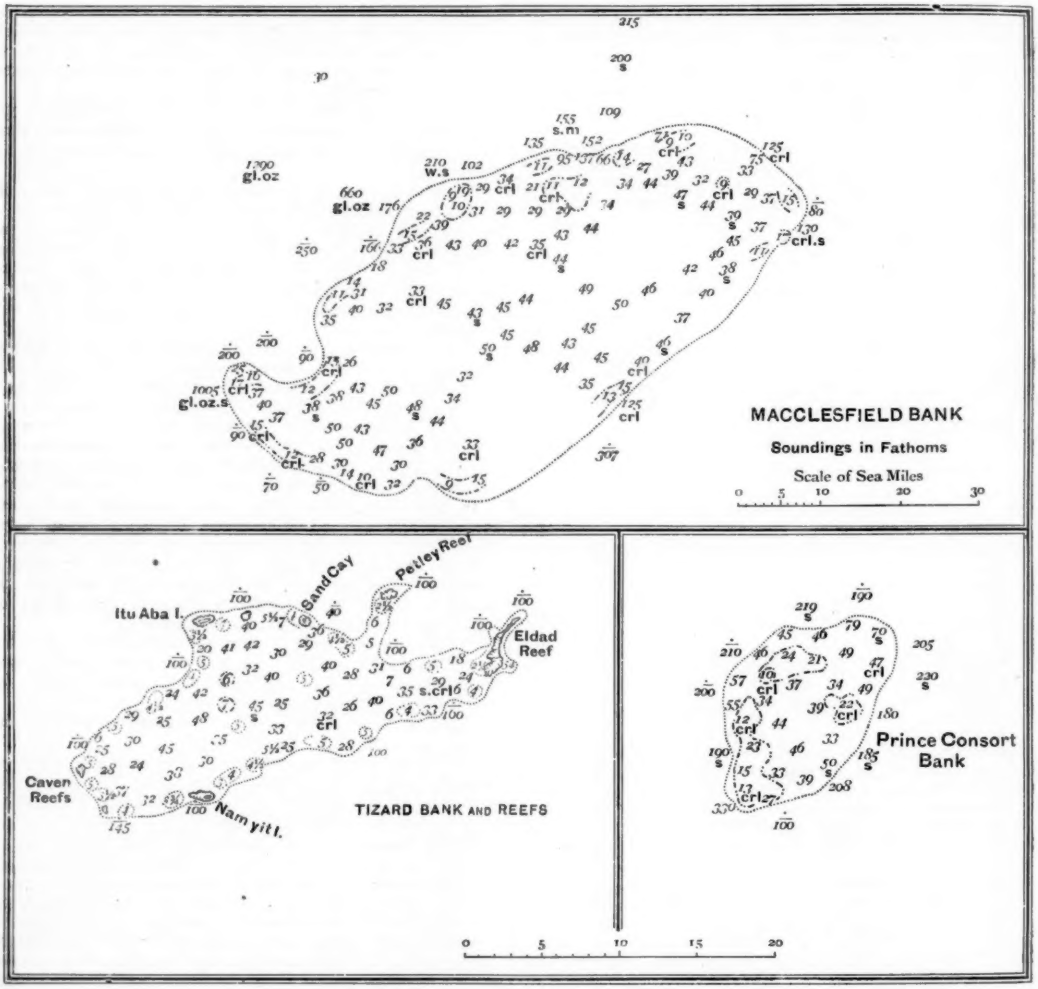
The evidence afforded by these reefs has probably escaped notice from the fact that as published in charts for the purpose of navigation they are mostly shown on a very small scale, in which their character is scarcely apparent. The original manuscript surveys in the records of the Hydro-

graphic Department being not generally available, no opportunity has been afforded for their examination, but on these their form is highly suggestive. It should also be remembered that few submerged banks have yet been surveyed on a sufficiently large scale and in sufficient detail to show their characteristics.

I may, however, add that it is well known to seamen experienced in navigation in coral waters that shallower soundings are frequently found on the edges of banks.

How precisely it comes about that coral is growing on the yet deep rims of these large banks, and that little or none is flourishing in the interior, evidence is yet wanting to show. These, however, are the facts, and the result, so far as the necessity for future scooping out is concerned, seems indisputable. I may nevertheless offer a suggestion.

This condition of reef is apparently only to be accounted for in two ways. Either by subsidence; or by assuming that the animals, be they corals or other lime-secreting organisms, that settle on the bank, do, when it gets, by their accumulation, within a certain distance of the surface, and under certain conditions of currents and food supply, intercept so much of the food borne in by the currents, that similar life, suitable to that depth zone, cannot be supported in the central area. Thenceforward, the rim alone will grow, and the organisms fitted to live in the successively shallower zones to the surface will only find on it foothold.



This would be the perfect atoll, but, with less nicely balanced conditions, growth would also take place in patches in the central area, as is so often the case.

It is to be observed that the depths on the more or less level tops of these large banks, as also in many perfect atolls, is frequently between 30 and 50 fathoms.

The smaller the bank, the less would be its general depth when the rim is formed, owing to the different proportions in which an area and its periphery, over which the non-consumed food must pass, relatively increase, and assuming that the animals that live nearer the surface consume more than those at greater depths.

I am fully aware that this view of the growth of an

atoll is not new. Darwin says, "A bank at the proper depth beneath the surface would give rise to a reef which could not be distinguished from an atoll formed during subsidence." Murray says, "Very early in the history of such an atoll, and while yet several fathoms submerged, the corals situated in the central parts would be placed at a disadvantage." It does not, however, appear to have been contemplated that the inclosed lagoon would, under any circumstances, without some further agency than the simple growth of the rim, be so deep as it appears to me, from the cases above cited, that it can be; nor, so far as I can find, have any such instances been before remarked.

Darwin notices the case of the Chagos Bank, but, on the authority of Captain Moresby, he states that the rim is dead, and concludes that life was killed by subsidence, and he apparently also infers that it will not grow to the surface.

I can find no fresh evidence on this particular reef, but for some of the banks in the China Sea I have the independent testimony of two of the officers employed in their survey, Staff-Commanders Tizard and Petley, R.N., that the coral growth is most luxuriant.

Touching for a moment on the point of the formation of banks at a proper distance below the surface; the investigation of banks in the Atlantic, recently described by Mr. J. Buchanan, shows that banks with almost wall-like slopes are growing up by the accumulation of organisms.

Turning to barrier reefs, similar instances can be found. Off the coast of the island of Palawan, a shallow rim is forming on the edge of the bank which stretches from the island to a distance varying from 15 to 30 miles, having a general depth on it of 40 fathoms. The length of the rim is 250 miles, and it consists of streaks and patches of coral with from 4 to 30 fathoms on them.

On the south of the eastern end of New Guinea, a formation, known to navigators as the sunken barrier, lines the edge of a bank similar to the above, and is of precisely the same character. Its length is 140 miles, and the depth between it and the land varies from 30 to 60 fathoms.

Dr. Guppy has recently pointed out some smaller instances of the same tendency in the Solomon Islands, and has made some remarks on the formation of barrier reefs in the same sense as my suggestions. I am not therefore advancing anything novel, but simply pointing out evidence which tends to show that the principle may be carried further than has hitherto been supposed.

Looking now at the fringing class, how comes it that so many wide reefs of this character exist, which, if Mr. Murray's contention be correct, should surely show more signs of the formation of a lagoon channel than they do?

Take the case of Rodriguez, in the Indian Ocean, with which I am personally intimately acquainted. Here a fringing reef surrounds the western sides of the island for a width of 4½ miles. There are narrow channels it is true, but so shallow that in many places boats cannot pass at low water. The island is situated in the heart of the strong trade winds, and the reefs are exposed to a heavy sea, which, with a rise of tide of nearly 6 feet, gives every facility for scour and rapid change of water.

I am not disputing the fact that calcareous dead organisms are dissolved by carbonic acid. I am no chemist, and moreover the *Challenger* observations amply prove it, but I would ask Mr. Murray if there is not a great difference between the position of small shells falling in water which completely surrounds them whilst they are constantly coming in contact with fresh particles of it, and of the more or less solid mass of a coral reef, which can only be attacked on its upper side to advantage, and where the resultant fine mud covers and protects the remaining rock, especially in the case of lagoon channels, when the bottom is partly composed of detritus from the land.

At the bottom of a lagoon of any depth, moreover, the motion of the water must often be comparatively slight, and the action consequently extremely slow.

The rotten state of the surface of every coral reef awash with the water shows that this disintegration is going on, but the fact that for large areas it remains awash, and must have so remained for ages, seems to me to point to the supposition that the removal of matter is too insignificant to account for the formation of deep lagoon channels in this manner, though doubtless it may explain the shallow pools and creeks found in all fringing reefs.

I have addressed myself solely to one point in this many-sided question, but I may add my opinion that, before any explanation which will fully account for the almost infinite variety of coral formations, can be given, much more knowledge of details of the complex conditions under which they may grow is required.

Certain knowledge of the depths at which different corals and other lime-secreting animals live under varying circumstances; of the amount of food carried in the different strata and in different waters; of the effect of the velocity of the currents that bring the food to the banks; and more accurate surveys on large scales, especially of the shapes and contours of coral reefs, and of their composition, are all wanting. These details must greatly affect coral growth, and the results must greatly vary. On the other hand, similar results may be brought about by different causes.

It may surprise some to learn how little in the preliminary matter of surveys alone has been done in the principal coral-reef regions, especially in the Pacific, which is generally quoted; and consequently how very inexact our knowledge is of the depth both inside and outside of the majority of atolls in the world, and also of the state of the bottom of the sea, on which it is very possible that many elevations may be found in the condition of those to which I have called attention.

In the Pacific the vast majority of islands have been merely sketched without a single sounding having been taken, either inside or outside lagoons.

I append a few statistics relating to the larger coral groups to show our position in this respect; merely remarking that the waters of the Fiji and the Society Islands are the only ones which can be said to be in any sense surveyed.

	No. of Islands.	No. surveyed.
Paumotu Islands	74	1 partially.
Ellice ,,	10	none.
Gilbert ,,	16	none.
Marshall ,,	30	none.
Caroline ,,	43	3 partially.
Tonga ,,	6 groups	2 groups partially.

W. J. L. WHARTON.

THE AKKAS, A PYGMY RACE FROM CENTRAL AFRICA.

AT the last meeting of the Anthropological Institute, Prof. Flower gave a description of two skeletons of Akkas, lately obtained in the Monbuttu country, Central Africa, by Emin Pasha, and by him presented to the British Museum. Since this diminutive tribe was discovered by Schweinfurth in 1870, they have received considerable attention from various travellers and anthropologists, and general descriptions and measurements of several living individuals have been published, but no account of their osteological characters has been given, and no specimens have been submitted to careful anatomical examination. The two skeletons are those of fully adult people, a male and a female, but unfortunately neither is quite complete. The evidence they afford entirely corroborates the view, previously derived from external measurements, that the Akkas

are among the smallest, if not actually the smallest, people upon the earth. There is no reason to suppose that these skeletons were selected in any way as exceptional specimens, yet they are both of them smaller than any other normal skeletons known, smaller certainly than the smallest Bushman skeleton in any Museum in this country, and smaller than any out of twenty-nine skeletons of the diminutive inhabitants of the Andaman Islands, of which the dimensions have been recorded by Prof. Flower in a previous paper communicated to the Institute. The most liberal calculation of the height of these two skeletons places that of the male at about an inch below 4 feet, and the female at less than an inch above. We may say 4 feet, or 1·219 metre, as the average height of the two, while a living female of whom Emin Pasha has sent careful measurements is but 1·164 metre, or barely 3 feet 10 inches. The results previously obtained from the measurements of about half-a dozen living Akkas are not quite so low as these, varying from 1·216 to 1·420, and give a mean for both sexes of 1·356, or 4 feet 5½ inches. Schweinfurth's original measurements were unfortunately lost, and the numbers since obtained are quite insufficient for establishing the true average of the race, especially as it is not certain that they were all pure-bred specimens.

In the list given in the third edition of Topinard's "Anthropologie" (1879) only two races appear which have a mean height below 1·500 metres, viz. the Negritos of the Andaman Islands 1·478, and the Bushmen 1·404. Of the real height of the former we have abundant and exact evidence, both from the living individuals and from skeletons, which clearly proves that they considerably exceed the Akkas in stature. That this is also the case with the Bushmen there is little doubt, although the measurements of this diminutive race are less numerous and carefully made.

The point of comparative size being settled, it remains to consider to what races the Akkas are most nearly allied. That they belong in all their essential characteristics to the black or Negroid branch of the human species there can be no doubt, in fact they exhibit all the essential characteristics of that branch even to exaggeration. With regard to the somewhat more rounded form of head (the cephalic index in these examples being 74·4 and 77·9 respectively), Hamy has long since pointed out that in equatorial Africa, extending from the west coast far into the interior, are scattered tribes of Negroes, distinguished from the majority of the inhabitants of the continent by this special cranial character, as well as by their smaller stature. The Akkas are grouped by Hamy and Quatrefages as members of this race, to which the distinctive name of "Negrillo" has been applied. Their small size has naturally led some anthropologists, including Schweinfurth, to ally them to the diminutive African race inhabiting the southern part of the continent—the Bushmen; but beyond certain characters met with in the whole Negroid branch, including the frizzly hair, there is little in common between them. The Bushmen are a very strongly marked race, and both their external appearance and osteological characters are so exceptional that they can never be confounded with any other. The natives of the Andaman Islands have also very distinctive characters, which they do not share with the Akkas, whose position all recent investigations show to be that assigned to them by Hamy as members of the Negrillo division of the Negroid branch of mankind. It is possible that these people gave origin to the stories of pygmies so common in the writings of the Greek poets and historians, and whose habitations were often placed near the sources of the Nile. The name "Akka," by which, according to Schweinfurth, the tribe now call themselves, has, singularly enough, been read by Mariette Pasha by the side of a portrait of a dwarf on a monument of the ancient Egyptian empire.

REV. JOHN HEWITT JELLETT, D.D., D.C.L.

IT is with extreme regret that we announce the death of the Reverend the Provost of Trinity College, Dublin. He died last Sunday evening after a very short illness.

The Provost was present in his usual health at the Spring Commencements, which were held on the 14th inst. in the Hall of Trinity College; on the 16th he was not quite well; on the 18th some dangerous symptoms appeared; and the end came, painlessly and unexpectedly, on the 19th inst.

John Hewitt Jellett was born at Cashel in the county of Tipperary on December 25, 1817. He entered Trinity College, Dublin, when seventeen years of age. Obtaining a Scholarship in 1836, he graduated as a Senior Moderator and Gold Medallist in Mathematics in 1837, and three years afterwards he obtained a Fellowship.

In 1848, he was appointed to the Professorship of Natural Philosophy; in 1870, on the death of the Rev. Dr. Luby, he was co-opted a Senior Fellow of Trinity College, and on the death of the Rev Dr. Lloyd, in 1881, he was nominated by the Crown to the Provostship.

Twenty years ago he was made one of the Commissioners of National Education in Ireland, and he was President of the Royal Irish Academy from 1869 to 1873.

In 1850, Jellett published his well-known "Treatise on the Calculus of Variations," a subject which had engaged the attention of some of the noblest mathematical intellects of the world. The volume contains improvements of previously existing methods, which, had they been given as so many separate treatises, would in themselves have formed no ordinary title to fame; but the author's aim was rather to compile a memoir which would enable the earnest student to be on a level with the knowledge he had himself attained to, however little that aim might be to his own glory.

For this work the Royal Irish Academy awarded Jellett in 1851 their Cunningham Gold Medal. In 1872 appeared the "Treatise on the Theory of Friction," a work well known and highly appreciated. In addition to these volumes a number of scientific memoirs were from time to time published by him in the Transactions of the Royal Irish Academy, and in Leonville's *Journal de Mathématique*, of which perhaps the more important were on the "Equilibrium and Motion of an Elastic Solid" and "On Researches in Chemical Optics."

Like other well-known mathematicians of the Dublin University, Jellett was as much thought of for his pulpit discourses as for his scientific memoirs. He was of good presence, had a clear articulation and a very persuasive style; and his appearance in the pulpit of the College chapel was always welcomed. When he dwelt on the moral difficulties of the Old Testament, none went their way without being impressed by the straightforward honesty of the man.

Occupying a very conspicuous position in a University city like Dublin, the Provost seemed to command the respect of all. Some perhaps there are who would have preferred that the head of so ancient a seat of learning should have taken no part in modern political strife; but, even amid the feverish excitement of party warfare, the Provost of Trinity College, though he often fearlessly and eloquently put forward his own views, did so without giving offence to any.

Intimately associated, from the year 1834, with the life of the College over which he at the last presided, the Provost knew much of, and was a prime mover in, many of the changes which have marked out a new life in the place. Within its walls he was known and respected, while the tribute to his memory paid by all classes of the Dublin citizens is a striking proof of how he was loved.

This is not the place to dwell on the intense loss his

death brings to those who had experienced the charm of that hidden inner life which was known to his friends as both kind and just, affectionate and sympathetic.

NOTES.

THE retirement of the veteran Prof. Prestwich from the Chair of Geology at Oxford is an event which cannot be chronicled without regret. But it is pleasant to know that he relinquishes the post which he has dignified for so many years to find in the quiet of his country home that leisure and rest to which his long devotion to the cause of science so justly entitles him. He has crowned his professorial career by the publication of the splendid volume which completes his great work on geology. On the very day after the appearance of that volume the electors met at Oxford to select from the numerous candidates a successor to fill his place. His University and the science of geology may both be congratulated on their choice. Prof. Green, whom they have chosen, is one of the most accomplished geologists in the country, one who has been trained in the practical school of the Geological Survey, who has done admirable original work, and who possesses in no common measure the power of luminous exposition. He is gifted, moreover, with a faculty in which geologists are often singularly defective, that of mathematical investigation, and we may hope that one of the results of his transference to Oxford will be to afford him an opportunity of devoting himself to the attack of many geological problems from the mathematical side. He carries with him to his new sphere of labour the best wishes of all to whom the progress of geology and the cultivation of science at the Universities are dear.

ON May 27 next Prof. F. C. Donders, of Utrecht, will be seventy years of age. The law requires that he shall then resign his duties as Professor at the University and as Director of the Physiological Laboratory, and it is thought that men of science in all parts of the world may be glad to take the opportunity of expressing their appreciation of the great services Prof. Donders has rendered to the study of physiology and physiological optics. An influential committee has been formed in Holland for the purpose of giving effect to this idea, and the proposal is that Prof. Donders' name should be connected in a permanent way with the spot where he has lived and worked for more than forty years, by the creation of a fund to be devoted to a scientific purpose, and to be known as the "Donders Memorial Fund." The uses to which the fund will be put, and the rules by which its administration will be governed, will of course be determined in accordance with the wishes of Prof. Donders. The Ophthalmological and Physiological Societies have taken the matter in hand in this country, and we have no doubt that the committees they have appointed will do their work satisfactorily. It ought not to be difficult for them to secure an adequate token of the respect felt in England for an illustrious man of science to whom the medical profession and the public are so deeply indebted. It is proposed that the amounts contributed by the several donors be not specified, but that they be grouped into a common sum for transmission to the Dutch Committee; and that the names of those contributing be inscribed in a suitable form for presentation to Prof. Donders. For this reason smaller as well as larger subscriptions will be acceptable. Subscriptions may be sent to Dr. Gerald F. Yeo, Secretary of the Committee of the Physiological Society (address—King's College, London, W.C.), or to Dr. W. A. Brailey, Secretary of the Committee of the Ophthalmological Society of the United Kingdom (address—11 Old Burlington Street, London, W.). Subscriptions may also be sent to the office of this journal.

AT a meeting of the Royal Society of Edinburgh, held on January 30, Profs. Clausius, Haeckel, and Mendeléeff were elected Honorary Fellows.

SIR JAMES PAGET, F.R.S., has consented to give the annual address to the students of the London Society for the Extension of University Teaching, at the Mansion House, on Saturday, March 3, at 3.30 p.m., under the presidency of the Lord Mayor. The subject of the address will be "Scientific Study."

A MARBLE medallion portrait of the distinguished palæontologist, Dr. Thomas Davidson, F.R.S., first chairman of the Brighton Museum Committee, was unveiled in the geological room of the Free Town Museum, Brighton, by the Mayor, Mr. E. Martin, on Friday, the 17th inst. The medallion, which is much admired, is the work of Mr. Brock, A.R.A. It was presented to the town on behalf of the subscribers by Mr. Edward Crane, chairman of the Museum Committee, who referred in detail to Dr. Davidson's services to science and to the Museum. Sir R. Owen sent a letter regretting that failing health prevented his paying the respect of personal attendance to the memory of his distinguished fellow-worker. Prof. Judd also wrote bearing cordial testimony to the skill and enthusiasm with which Dr. Davidson carried on his researches.

GENERAL PERRIER, the most eminent French authority on geodesy, died at Montpellier on Monday at the age of fifty-five. He had attained the rank of Brigadier-General in the French army, and was at the head of the Geodetic Department at the War Office. He was a member of the Academy of Sciences.

THE relations of science and religion do not form one of those topics which we permit ourselves to discuss in NATURE. At the same time we may call attention to a series of three remarkable articles on "Darwinism and the Christian Faith" recently published in the *Guardian* (January 18, January 25, and February 1, 1888), and now reprinted as a pamphlet. The author is anonymous, but is understood to be an Oxford College tutor, and Honorary Canon of Christ Church. The orthodoxy of the *Guardian* is, we believe, unimpeachable. We notice therefore with gratification that not only is Darwinism thoroughly accepted and lucidly expounded by the writer in the *Guardian*, but that he is an exceptionally well-informed and capable critic, whose scientific knowledge is varied and sound. The publication of these articles in the *Guardian* is a proof that the clergy as a body are not so unwilling to accept new scientific views as might be supposed were we to regard Dean Burgon as a fair sample of his class. The *Guardian's* contributor discusses the difficulty of reconciling the existence of a just, omnipotent, and omniscient God with the existence of pain and the ceaseless "struggle for existence," and *à propos* has a remark tinged with local colour which is worth reproducing. "And yet," he says, "man, who is so wise and good that he is always saying, with King Alphonso of Castile, 'If God had called me to His councils things would have been in better order,' has invented competitive examinations, which mean suffering and pain for all, without even a compensating 'survival of the fittest' or improvement of the race!" We believe that competitive examinations were invented by the Chinese, and introduced into Europe by Jesuit missionaries. The Chinese are celebrated among the nations of the world for the elaborate system of cruel tortures employed in their administration of justice. On the other hand, we owe tea and many other nice things to them.

THE annual winter meeting of the Department of Superintendence of the U.S. National Education Association was held lately at Washington. The most important topics treated were "How and to what extent can Manual Training be ingrafted on our System of Public Schools?" and "How can the Qualifications of Teachers be determined?"

THE Archæological Congress which is to be held at Moscow, in 1890, will have an international character, many German, French, Italian, and other men of science having already been invited to attend. The Congress will consider the following subjects: (1) prehistoric antiquities; (2) historical, geographical, and ethnographical questions; (3) Russian art monuments; (4) ecclesiastical monuments; (5) Slavo-Russian language and writing; (6) Slavo-Byzantine and West European antiquities; (7) Oriental and heathen antiquities; (8) latest progress of archæology.

MRS. ZELIA NUTTALL has been elected a Fellow of the American Association for the Advancement of Science in recognition of her researches in Mexican archæology.

A TRANSLATION of Dr. E. B. Tylor's hand-book of "Anthropology" into Spanish by D. Antonio Machado has just been published in Madrid. The author contributes a special preface drawing attention to the valuable anthropological material still to be found in Spanish America.

THE first number of what will no doubt prove to be an important and valuable periodical has just been issued. It is called *Internationales Archiv für Ethnographie*, and is edited by J. D. E. Schmeltz, of the Ethnographical Museum of Leyden, who has received promises of co-operation from many of the foremost ethnologists and anthropologists in Europe and America. The present number (which contains German, Dutch, and French contributions) opens with a striking article, in German, by Dr. L. Serrurier, Director of the Leyden Ethnographical Museum, on the arrows of New Guinea. Representations of the various types of New Guinea arrows, admirably printed in colours, illustrate this interesting paper.

WE have received the first number of *The American Anthropologist*. This new quarterly periodical is issued under the auspices of the Anthropological Society of Washington, and the editorial committee seek the co-operation of all who are interested in the advancement of anthropological science. In the first number there are papers on the law of Malthus, by Dr. James C. Walling; the development of time-keeping in Greece and Rome, by F. A. Seely; the human hand, by Dr. Frank Baker; and the Chane-abal (four-language) tribe and dialect of Chiapas, by Dr. D. G. Brinton.

THE *American Meteorological Journal* for January contains:—(1) An article by F. Waldo on instruments for measuring atmospheric pressure, showing the differences that exist in the standard barometers of different countries, and that the standards do not always remain constant for a number of years. (2) A paper by Prof. H. A. Hazen on the exposure of thermometers, with a discussion of a new plan proposed by Dr. R. Assmann. The latter paper was presented to the Berlin Academy in November last. (3) On a thirty-day period of the weather, by H. Helm Clayton. The writer considers that the period is strongly substantiated by facts, although at present they remain empirical facts.

THE Deutsche Seewarte has published the seventh volume of the results of meteorological observations for 1° squares of the North Atlantic Ocean. The object is to discuss the observations collected by German and Dutch vessels between latitude 50° and 20° N. in the North Atlantic, adjoining the district of the nine equatorial 10° squares between latitude 20° N. and 10° S., and longitude 10° and 40° W., the observations for which have been discussed by the Meteorological Council. The district now covered by the two institutions embraces 60° of latitude and 30° of longitude, with the exception of one 10° square, which will shortly be published. The German observations are published in a tabular form, showing for each degree the direction of the winds under sixteen points, the number of storms, the mean wind force, pressure, temperature of air and sea, rainfall, and other particulars. The number of observations for each subdivision is sometimes small, but always quoted, and in their present form the observations may be added to subsequently, or amalgamated with those of other countries. A small but important district south of 20° N. (the limit of the German investigation) and west of 40° W. (the limit of the English investigation), embracing the region of the origin of the West India cyclones, has yet to be undertaken by some body, to complete these important contributions to maritime meteorology.

A NEW chloride of gold, Au_2Cl_4, has been prepared by Prof. Julius Thomsen, of Copenhagen (*Journ. für Prakt. Chemie*, 1888, No. 2). The method of preparation is remarkably simple, gaseous chlorine being merely brought into contact with gold in a fine state of subdivision and at a moderately elevated temperature. About 50 grammes of finely divided gold, obtained by precipitating a solution of the trichloride with sulphurous acid, was thoroughly washed partially dried to the consistency of a thick mud, and placed in a previously weighed glass tube. At its lower end the tube was drawn out and the delivery tube of a chlorine generator sealed on to it. A stopper and exit tube at the upper end completed the arrangement. A rapid stream of chlorine gas (half a litre per minute) was then passed through the apparatus, the lower end of the wider portion containing the gold being gently heated to start the reaction. The whole was then placed in a glass beaker surrounded by cotton-wool in order to prevent too rapid cooling, by which device the heat of the reaction itself was sufficient to complete the combination. At the commencement the absorption of chlorine was perfect, not a bubble escaping, but at the expiration of half an hour the point of saturation was reached. After expelling the uncombined chlorine the tube was again weighed, and the amount of chlorine thus taken up determined. In every experiment the proportion of chlorine to that of gold was found to be very slightly more than two to one, the average ratio being 2·09 to 1·0. The slight excess of chlorine was due to minute spangles of trichloride of gold sparsely disseminated throughout the mass. The simplicity of this mode of preparation and the constancy of the results may perhaps excite wonder that Au_2Cl_4 has not hitherto been as well known as $AuCl$ and $AuCl_3$. As a matter of fact, Prof. Thomsen discovered it several years ago, and published his results, but owing to the adoption of different methods by later workers the conclusions of Prof. Thomsen were not considered confirmed. Now that the work has been repeated and completely verified there is no longer any reason why Au_2Cl_4 should remain in the background. It is interesting theoretically as being the aurous salt of chlor-auric acid, $HAuCl_4$.

FROM the Annual Report of the New York State Department of Public Instruction, it appears that during last year over 31,000 teachers were employed in the State of New York, and that of this number only 5821 were males. The number of children of school age was 1,763,115; the total enrolment, 1,037,812; the average attendance, 625,610. Mr. Draper, the Superintendent, the author of the Report, says that the attendance in the schools does not keep pace with the growth of the population, and that the uneducated class is increasing.

COLONEL LE MESSURIER has just brought out a third edition of his useful pocket hand-book on the "Game, Shore, and Water Birds of India." The utility of this unpretending little work has been vouched for by the call for its re-issue in an octavo form with the addition of many drawings made by the author during his recent furlough in England, and we are glad to see to what practical use he has turned some of Prof. Flower's exhibits at the Natural History Museum. Colonel Le Messurier writes as a field-naturalist for field-naturalists and sportsmen, without any

great pretensions to scientific knowledge, but there is no doubt that all naturalists will gain useful hints from this little volume, which is profusely illustrated with woodcuts, giving the characteristic features of most of the species.

"THE STATESMAN'S YEAR-BOOK" for 1888 has been published. It contains additions and alterations which largely increase the value of the work, and the statistical and other information has been brought up to the latest available date.

WE have received three issues of the "Annuario" published by the Imperial Observatory at Rio de Janeiro—the issues for 1885, 1886, and 1887. The work is well compiled, and the editor evidently takes great pains to secure that each issue shall be decidedly better than its predecessors. Besides the usual collection of astronomical facts, the work contains useful tables relating to meteorology, chemistry, physics, geography, and other sciences.

THE "Annuaire Géologique Universel" of Dr. Dagincourt, which has just been issued for the third time, has been much enlarged and improved. The new volume contains an exhaustive review of recent work in palæontology and geology.

A FOURTH edition of Prof. Nichol's "Tables of European History, Literature, Science, and Art, from A.D. 200 to 1888" (Maclehose) has been issued. The idea of the work is good, but we cannot say that the scientific tables are always quite satisfactory. In his lists of men of science the compiler includes the names of some writers who have a very inadequate claim to the place he accords to them.

THE Trustees of the Australian Museum have issued a descriptive catalogue, by Dr. R. von Lendenfeld, of the Medusæ of the Australian seas. Speaking of the Scyphomedusæ, Dr. von Lendenfeld says that he has observed three species in New Zealand, three species on the coast of Victoria, and five species in Port Jackson. Two of the latter are identical with the Victorian species. Of the nine species, six have been described by Dr. von Lendenfeld; his specimens of the remaining three were not sufficiently well preserved for description. The difficulty connected with the preservation of these beautiful animals has, he points out, been a great obstacle in the way of a thorough knowledge of them.

THE fifth volume of the collected works of Paul Broca has just been issued by M. Reinwald, in Paris. This volume, which, like the others, is sold separately, is particularly interesting to zoologists. It contains Broca's numerous and important memoirs on the brain of man and primates. It is well illustrated.

M. ZOGRAFF'S new work on the structure of the *Acipenser ruthenus*, which appeared in the *Izvestia* of the Moscow Society of Amateurs of Natural Science (vol. lii. fasc. 3), will be most welcome to zoologists. Following the methods adopted by Günther, Johann Müller, and Pallas, and more especially by the Swedish ichthyologist, F. A. Smitt, who has applied the system of numerous measurements used in anthropology to the study of fishes, M. Zograff has undertaken to give anew a complete description of the Russian species of *Acipenser*, and the Central Asian species of *Scaphirhynchus*. He begins his work by a general description of the body of the Russian Acipenseridæ: the varying shapes of their heads; the indexes of length, width, and thickness of the body; the skin and spines; the teeth; the muscles; and the brain. The whole is accompanied by numerous engravings and coloured plates, great attention being given to the minute anatomy of all parts of the different species.

AMONGST the papers contained in the last issue of the Transactions of the Seismological Society of Japan (vol. xi.) is one on earth tremors in Central Japan, by Prof. Milne. The paper is a continuation of one on the same subject read before the same Society in 1883, which was referred to in these columns at the time. In the present paper the writer discusses recent investigations into earth tremors in Italy, describes tremor recorders, with special reference to an automatic tromometer, gives numerous tables of records of the latter instrument, and finally refers to the subject of earth tremors on mountains. The paper is one of great length, and is accompanied by numerous charts and tables, which make it a respectable volume in itself. The conclusions may, however, be given in a brief space. Prof. Milne says that his chief object has been to show the relationship which earth tremors hold to barometrical fluctuations, barometrical gradients, and the wind. He concludes that they are more frequent with a low than a high barometer, but even with the former they may often not be observed; that with a high gradient they are almost always observed, but with a small gradient only seldom: that the stronger the wind the more likely they are to be observed; when there has been a strong wind and no tremors it has often been a local wind, or one blowing inland from the Pacific Ocean; the recorded earthquakes do not appear to be connected with earth tremors, more than that both are more frequent at the same seasons; and tremors are as severe on the summit of a lofty mountain as on the plains. So far as his observations have hitherto gone in Japan, it appears that the majority of earth tremors are movements produced by the action of the wind upon the surface of the earth, and that these may often be propagated to distant places where wind disturbances have not occurred.

ON January 13, at 11.10 p.m., a faint shock of earthquake was felt in the district of Örebro, in Central Sweden. It was not accompanied by any subterranean noise.

AT five o'clock on Sunday afternoon a cyclone broke over Mount Vernon, a town in Illinois, sixty miles to the southeast of St. Louis. Many persons were killed or injured, and five hundred buildings were demolished in a few minutes. The cyclone is said to have come up from the south-west with a rotary whirling motion, sweeping a path five hundred yards wide and several miles long, within which everything was destroyed.

DURING last month the so-called "red after-glow" was observed at sunset in the vicinity of Stockholm. Varying in intensity the glare extended considerably towards the zenith.

THE Finnish Government is on the point of organizing a number of stations along the coast of Finland for the observation of the nature and peculiarities of the drift-ice during the winter months.

A LARGE block of stone with rude drawings and some Runic inscriptions has just been discovered in the island of Tjörn, on the south-west coast of Sweden. It is of particular interest as being the first of its kind found in the southern part of the province of Bohus.

A MEETING has just been held at Tönsberg, in Norway, of those interested in the Arctic seal fisheries in that country, for the purpose of considering the Scottish Fishery Board's proposals that the close time for seal should end on April 10 instead of as at present April 3, and begin on July 10 instead of July 15. Both proposals were unanimously rejected, the reason advanced being that their adoption would tend to ruin the industry, so far as Norway is concerned. The Scotch proposal that young and old seal should be treated alike during the open season was adopted. Finally, the following resolutions were passed: (1) that it was advisable that the close season should end at 6 a.m. on April 3 instead of at midnight; (2) that the law of preservation of seal should be altered so that the area covered by it should range from 60° to 70° N., and from 10° E. to the coast of Greenland. A report of the meeting will be forwarded to the Scottish Fishery Board for their consideration.

THE Norwegian Fishery Promotion Society of Bergen has petitioned the Government for a grant of £15,000 for the development of the deep-sea fisheries of Norway.

THE Merchant Taylors Company have recently voted ten guineas to the Parkes Museum to aid in its work of practical teaching and demonstrating sanitary science.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀), two Rhesus Monkeys (*Macacus rhesus* ♀ ♀) from India, an Alpine Marmot (*Arctomys marmotta* ♀), European, a Vulpine Phalanger (*Phalangista vulpina* ♂) from Australia, presented by Mr. H. Austin Clow, F.Z.S.; three Esquimaux Dogs (*Canis familiaris*) from Greenland, presented by Mr. W. T. Tournay, F.Z.S.; three Derbian Wallabys (*Halmaturus derbianus*) from Australia, presented by Lieut. C. M. Hepworth, R.N.R.; four Alpine Accentors (*Accentor alpinus*), European, presented by the Lord Lilford, F.Z.S.; a Cardinal Grosbeak (*Cardinalis virginianus*) from North America, presented by Mr. Ayerst; four Lion Marmosets (*Midas rosalia*), an Eyra (*Felis eyra*) from Brazil, four Parrot Finches (*Erythrura psittacea*) from New Caledonia, two Common Gulls (*Larus canus*), a Black-headed Gull (*Larus ridibundus*), British, purchased; four Cereopsis Geese (*Cereopsis novæ-hollandiæ*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

MR. TEBBUTT'S OBSERVATORY, WINDSOR, NEW SOUTH WALES.—Mr. John Tebbutt, the well-known and enthusiastic amateur astronomer of New South Wales, has just published a little pamphlet giving a history and description of his private observatory, the work of which, he remarks with justifiable pride, "has proved of sufficient importance to admit of Windsor being placed in the list of observatories in the British and American *Nautical Almanacs*, the *Connaissance des Temps*, and the *Berliner Astronomisches Jahrbuch*." And this distinction has been well earned, for the lists here given of observations made, and of papers contributed to various scientific publications, show the history of the little observatory to have been a most honourable one. Mr. Tebbutt has made all the observations himself, and until 1881 performed all the reductions; latterly he has received occasional assistance in the computations from his son or friends near. His instrumental equipment was for many years of the most modest description: for seven years it consisted principally of a sextant, and a telescope of $1\frac{3}{4}$ inches aperture. In 1861 a refractor of $3\frac{1}{4}$ inches aperture, and in 1864 a transit instrument of 2 inches, were added. In 1872, Mr. Tebbutt became the possessor of an equatorial of $4\frac{1}{2}$ inches, which was his chief instrument until about a year and a half ago, when he bought a fine 8-inch equatorial by Grubb, once the property of the late Dr. Bone, of Castlemaine. The observations made have been principally of comets, for a number of which Mr. Tebbutt has also computed orbits, but daily meteorological observations have been kept up for twenty-five years, the results of which have been published in five parts, and transit observations have been taken regularly for time. Mr. Tebbutt has also done good service to science by his papers on astronomical subjects in various organs of the colonial press, for hitherto the private pursuit of astronomy has been greatly neglected in the Australian colonies, and he has almost stood alone as an amateur observer. It is to be hoped that this record of his labours and his success may call forth many imitators.

PULKOWA OBSERVATORY.—The Report of this Observatory for the year ending May 31, 1887, refers to the heavy loss the institution sustained in the death of Dr. August Wagner. Owing to his death, the work of publication has been somewhat delayed; he had, indeed, finished a memoir on personal and instrumental errors for the introduction of vol. xii., but the materials he left for the stellar and planetary catalogues were not so readily dealt with. Still, it is expected that this volume, and the introduction to vol. xiv., may soon be ready for publication; vols. xv. and xvi., which will contain meridian observations for the period 1872–80, and the catalogue, are ready to follow vol. xiv. through the press. Of vol. viii., the catalogue, forming the first part—meridian observations 1840–69 of Bradley and other stars down to mag. 6—has already been distributed, and the remainder is in hand; vol. x., Prof. Struve's double-star observations, is still incomplete. The observational work of the Observatory has suffered no great change. The 30-inch refractor has been used by Dr. H. Struve for the measurement of the more difficult of Burnham's stars, the fainter satellites of Saturn, and the satellite of Neptune. The old 15-inch has been used by Drs. H. Struve and Hasselberg for photographic experiments, and by Prof. O. Struve for observations of Procyon, which has now been followed through nearly a complete period of its orbital motion.

WOLSINGHAM OBSERVATORY.—The Rev. T. E. Espin reports that during the last year he has continued his sweeping for red stars and stars with remarkable spectra, and that he has published spectra of 126 objects in the *Astronomische Nachrichten*, Nos. 2788 and 2825, of which eighty-six were found in the sweeps. Fifteen of the stars were of Secchi's type IV. Three new variables of long period have been discovered, and the usual observations of variables have been made and forwarded to Prof. E. C. Pickering. The Observatory has been enriched by the present from Canon Slatter of a fine 4·8-inch equatorial by Troughton and Simms. The new edition of Birmingham's Red Star Catalogue will be ready for the printer in a few weeks.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 FEBRUARY 26—MARCH 3.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 26

Sun rises, 6h. 54m.; souths, 12h. 13m. 10·0s.; sets, 17h. 32m.: right asc. on meridian, 22h. 36·3m.; decl. 8° 48′ S. Sidereal Time at Sunset, 3h. 56m.

Moon (Full, February 27, 12h.) rises, 16h. 22m.; souths, 23h. 50m.; sets, 7h. 4m.*: right asc. on meridian, 10h. 15·3m.; decl. 12° 53′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	6 53 ...	12 50 ..	18 47 ...	23 13·3 ...	1 26 S.
Venus ...	5 39 ...	9 58 ..	14 17 ...	20 20·4 ...	19 26 S.
Mars ...	22 15* ..	3 33 ...	8 51 ...	13 55·0 ...	8 58 S.
Jupiter ...	1 40 ...	5 53 ...	10 6 ...	16 14·6 ...	20 17 S.
Saturn ...	13 50 ...	21 47 ...	5 44* ...	8 11·6 ...	20 35 N.
Uranus ...	21 8* ...	2 41 ...	8 14 ...	13 2·6 ...	5 56 S.
Neptune..	9 38 ...	17 18 ...	0 58* ...	3 42·0 ...	17 57 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4 ...	81 16 N. ...	Feb. 29,	18 56	*m*
Algol	3 0·9 ...	40 31 N. ...	Mar. 1,	3 12	*m*
λ Tauri...	3 54·5 ...	12 10 N. ...	,, 3,	1 28	*m*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	Feb. 28,	22 0	*m*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,, 29,	19 18	*m*
			Mar. 1,	22 34	*m*
U Monocerotis ...	7 25·5 ...	9 33 S. ...	Feb. 26,		*m*
δ Libræ	14 55·0 ...	8 4 S. ...	,, 29,	1 32	*m*
R Ophiuchi... ...	17 1·3 ...	15 57 S. ...	,, 28,		*M*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	Mar. 1,	0 42	*m*
		and at intervals of		20 8	
X Sagittarii... ...	17 40·5 ..	27 47 S. ...	Feb. 26,	2 0	*M*
W Sagittarii ...	17 57·9 ...	29 35 S. ...	Mar. 2,	5 0	*m*
R Lyræ	18 51·9 ...	43 48 N. ...	,, 2,		*M*
U Aquilæ	19 23·3 ...	7 16 S. ...	,, 3,	5 0	*m*
S Aquilæ	20 6·5 ...	15 17 N. ...	Feb. 26,		*m*
Y Cygni	20 47·6 ...	34 14 N. ...	,, 27,	19 24	*m*
			Mar. 1,	19 18	*m*
S Cephei	21 36·6 ...	78 7 N. ...	,, 3,		*M*
δ Cephei	22 25·0 ...	57 51 N. ...	,, 1,	21 0	*M*

M signifies maximum; *m* minimum.

Occultation of Star by the Moon (visible at Greenwich).

Feb.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
26	7 Leonis	$6\frac{1}{2}$	5 16	6 7	112 291

March. h.
1 ... 23 ... Mars in conjunction with and 2° 37′ south of the Moon.
3 ... 19 ... Mercury in inferior conjunction with the Sun.

Meteor-Showers.

	R.A.	Decl.	
Near δ Virginis	192°	2° N.	March 2 and 3.
From Sagittarius	280	17 S.	Very swift; streaks.

THE RELATIONS BETWEEN GEOLOGY AND THE BIOLOGICAL SCIENCES.[1]

IN the remarks which at our last anniversary I had the honour of offering from this chair, I congratulated the students of geology and mineralogy upon the new and intimate relations which, to their mutual advantage, are now growing up between those departments of science. It has, however, been suggested that, while geologists are thus being brought into closer alliance with mineralogists, the strong bonds of union which have so long united us with the biologists are becoming somewhat relaxed, and, indeed, stand in no small danger of actual dissolution.

Highly as I estimate the value of the *rapprochement* between the geological and mineralogical sciences, I for one should regard such a result as far too dearly purchased, if it necessarily involved any interruption of the close relations which have so long subsisted between geology and biology. But I cannot for one moment believe that such a grievous misfortune seriously threatens the cultivators of the two great departments of natural science.

Notwithstanding certain divergencies of opinion which have made themselves heard within an ancient University, and have awakened a faint echo in the halls of our National Museum, I cannot doubt that the teachers of geology and biology will easily discover a *modus vivendi* upon what is, after all, a subject of very secondary importance—the arrangement of natural-history collections.

No one can read recent declarations of the present Director of our National Museum without being impressed by his manifest desire to make the splendid collections under his care reflect, as completely as possible, the present condition of our knowledge of natural history. And if, on the other hand, we turn to the remarks made by the Keeper of the Zoological Department, at Swansea, in 1880, and to those of the Keeper of the Palæontological Department, at Manchester, last year, we shall find in those utterances ample guarantees that, in the arrangement of their collections, questions of practical convenience will not be lost sight of; we shall be satisfied that there is not the smallest danger of revolutionary ideas leading to the removal of "ancient landmarks," or of unattainable ideals being sought through the wholesale commingling of incongruous elements. The collections of our Universities are happily free from the conditions which must always hamper an institution where the interests of popular amusement have to be reconciled with those of scientific work; and it is for the teachers of natural science in those centres of thought to agree upon an arrangement which may best serve to illustrate their courses of instruction.

But while the discussion on museum-arrangement may be regarded as a purely academical one, which, after scintillating for a while in letters and pamphlets, died out in some not very formidable explosions at the recent meeting of the British Association, it may be wise on our part not to pass by quite unnoticed some indications of the attitude of the younger school of biologists towards palæontological science, this attitude having been very conspicuously manifested during the discussion in question.

If I rightly apprehend the views of some of my biological friends, as gathered not only from their published utterances, but also from private conversations, the position they are inclined to take up may be expressed somewhat as follows:—

"Palæontology has no right whatever to separate existence as a distinct branch of science. Fossils are simply portions of animals and plants, and ought to be dealt with as such; for all scientific purposes it is quite immaterial whether the organism which we are called upon to study expired only an hour since or died millions of years ago. Imperfect fragments can only be properly interpreted in the light afforded by the more complete structures found in recent organisms; and hence the naturalist who is engaged in studying a particular group of living organisms is the only person competent to deal with its fossil representatives. In our laboratories and our museums alike, therefore, fossil remains ought to be studied side by side with the living types which most nearly resemble them, and always by the same investigators. This being the case, it is neither necessary nor expedient that there should be a class of students whose chief concern is with extinct forms of life; and as for the geologists, they have really no farther concern with fossils than just to find them, attach a label indicating the period at which they must have lived, and hand them over to the biologist for study and incorporation in his collections. Any action beyond this can only be regarded, indeed, as an act of usurpation on the part of geologists, and must tend, not to the advancement, but to the injury of true science."

Such, so far as I have been able to gather them, are the extreme opinions which some biologists now entertain. It may, perhaps, seem presumptuous on my part to venture to offer a plea for palæontology, but there are considerations which may induce us to regard such a plea as coming better from one whose place in the ranks of the geological army lies nearer the centre than in the biological wing; from one who regards palæontology as the borderland of the geological and biological sciences—a borderland where the cultivators of both ought ever to meet, not for rivalry and aggression, but for the necessities of intellectual commerce and the advantages of mutual help.

The view of palæontology which I have ascribed, I believe not unjustly, to some biologists is one which has just such an amount of truth in it as to render it plausible, but at the same time, as I cannot but believe, is one of those half-truths which are proverbially more dangerous than downright errors. Palæontology is not, as has often been confidently asserted, simply a branch of biology; it is equally a part of geological science, and there are the strongest grounds, both of reason and expediency, for retaining it in that position. All geological science is based on the principle that the past can only be interpreted by the study of the present; Darwin was the intellectual child of Lyell, and the "Origin of Species" was the logical outcome of the "Principles of Geology." No palæontologist, worthy of the name, has ever dreamed of studying fossils except in the light afforded by the investigation of their recent analogues. Indeed, if we were to carry out the aggressive ideas of some biologists to their legitimate consequences, there would be left to us no science of geology at all; for why, it may be asked, should the study of physical processes in the past be carried on separately from the investigation of the same processes as exhibited at the present time? But then, by a strange Nemesis, I fear the same all-devouring physics, after swallowing up geology, would make very short work indeed with biology itself. And there is still in the background another claimant for universal empire in the realms of thought, for are there not some who dream of all sciences ultimately becoming the victims of that new portent of ambition—"geography"?

In considering the present position and future claims of palæontology, I may be permitted at the outset to offer a protest against a class of objections which has sometimes been very unfairly urged against the votaries of that branch of science. It has often been assumed that the students of fossils are contented with a lower standard of excellence than that which is aspired to by the cultivators of other branches of natural history. Now, setting aside for a moment the very important consideration that, owing to the imperfection of the remains which they are called upon to study, palæontologists are confronted by difficulties which do not beset the investigators of recent forms, I maintain that the charge is an altogether unjust one. Palæontologists are no more responsible for the unwise use made of fossils by incompetent persons than are zoologists for the vagaries of shell- and butterfly-hunters, or botanists for the absurdities of fern- and diatom-collectors.

Doubtless there has been much work done in connection with

[1] Address to the Geological Society by the President, Prof. John W. Judd, F.R.S., at the Anniversary Meeting, on February 17.

fossils, as well as with other natural history objects, of which we can only speak with shame and regret as having been undertaken unadvisedly and performed ignorantly,—work which, prompted by an unwise ambition, has been conceived in error and brought forth in presumption.

It would ill become anyone from this chair to speak lightly of the great, the inestimable services rendered to our science by the collectors of fossils. How many interesting and novel forms have been brought to light by their patient efforts! How often has the structure of obscure types been rendered clear through their constant and persevering endeavours to obtain more perfect specimens! Yet sometimes the very zeal of collectors has led them astray. Despairing of finding systematic zoologists and botanists who could devote the necessary time and attention to the study of objects which they have obtained with so much trouble and pains, they have unwisely undertaken, without the necessary training and knowledge, the naming and description of forms of life which required for their proper interpretation all the skill and experience of the most able comparative anatomist or vegetable morphologist.

I feel sure that, if those who have thus erred, through acting with "a zeal which is not according to knowledge," could realize the injury done to science by such proceedings, they would pause before burdening scientific literature with premature names, imperfect diagnoses, and ill-digested materials. Fossils are, it is true, "the medals of creation," and for the purposes of the historian of past geological times, it may seem that any name, however bad, which can be employed for purposes of reference must be better than none at all. But fossils, it must be remembered, are much more than mere "medals." They are the precious relics of the faunas and floras of bygone times; landmarks—the only ones we can ever hope to discover—which may serve to guide us in tracing the wonderful story of the evolution of the existing forms of life. Reverently—as the mineralogist treats meteorites, those pocket-planets and errant members of the outer universe—should the biologist regard fossils, the fragments of an earlier life, the collateral, if not the direct, ancestors of living types.

So far I am from thinking that the study of fossils ought in all cases to be undertaken by those who are actually engaged in working out their recent representatives, that I believe such a practical abolition of palæontology as a distinct branch of science would tend, not to the advantage, but to the injury, of both biology and geology. And I will venture to set forth my grounds for this conclusion.

It may be remarked at the outset that at a time when all the tendencies of biological science appear to be towards an extreme specialization, it is strange to find that there are advocates for the suppression of what is now so well-developed a department of biological science as palæontology. When the work to be done has become so vast that some biologists feel themselves compelled to restrict their studies and labours to the morphological, or even to the histological department, others to the embryological, the physiological, the taxonomic, or the chorological branches of zoology or botany respectively, why should not some concentrate their efforts upon the elucidation of the ancient forms of life? When the study of a single group, often a very limited group, of animals or plants is sufficient to exhaust the energies of a particular naturalist, it is surely not unreasonable that forms which have become extinct and have left only very imperfect evidence of their structure and affinities, and these requiring peculiar methods for their study, should attract the attention of special investigators.

The study of fossils, we may remark, if it be undertaken by any biologists, must fall to systematic zoologists and botanists, and these have become somewhat rare and out of fashion in modern times; so few in numbers, indeed, do they seem as to be scarcely able to cope with the ever-increasing array of living forms; and it would be a hopeless task if upon them were also cast the overwhelming mass of fossil ones.

Imagine the embarrassment and dismay of a student of living sponges, whose favourite (possibly his only) method of research has consisted in studying with the microscope innumerable thin slices cut from tissues and embryos, if a cartload of chalk-flints were thrown down at his door, and he were required to interpret the fragments of sponge-skeletons which they contained in every conceivable variety of disguise through peculiar processes of mineralization!

There are, indeed, a variety of special reasons why ordinary systematic zoologists and botanists become, by the very habits acquired in their daily pursuits, singularly ill fitted for dealing with fossil forms.

In studying recent forms the zoologist or botanist is bound to take into consideration, in fixing the systematic position of an organism, not only its skeleton, but all its soft parts, and even the structure and mode of development of its embryo; he may also be called upon to note physiological peculiarities, before he is in a position to arrive at a decision as to its place in the zoological or botanical series. But for the student of fossil forms none of these aids are available, he is compelled to do his best without them. Investigators of the recent Mollusca are, of course, "malacologists," but he who studies the extinct forms of the group must perforce labour under the stigma of being "a mere conchologist." In examining recent vertebrates it is allowable to make every possible use of the aid afforded by a study of the ligamental skeleton, in unravelling their affinities; but he who works on fossil vertebrates is and must remain a pure osteologist. Botanists have been led to the conclusion that for the classification of plants the reproductive organs always afford the safest guides; but palæontologists, alas! are frequently called upon to do their best in deciphering fragmentary remains of the vegetative organs.

It is not, as some biologists would almost seem to imagine, that palæontologists are led by any perversity of mind to reject the light which is afforded to them, or that they are not deeply sensible of the great value and importance of many recent researches in respect to living forms; but simply that they realize—often very sadly realize—the impossibility of availing themselves of the help afforded by such researches, in connection with the very imperfect material with which they are called upon to deal.

If we were to suppose that a surveying ship brought home from a newly-discovered island a heterogeneous mixture of isolated bones and teeth, of shells, bits of stick and fallen leaves, zoologists and botanists might be perfectly justified in refusing to waste their time upon such unsatisfactory materials. But if, subsequently, news arrived that after the departure of the ship the whole island had sunk beneath the ocean, then the circumstances would have completely changed, and no pains and care would be felt to be too great if expended in dealing with such a unique collection, however imperfect it might be. Or, to take a case which has actually occurred, the curators of the Ashmolean Museum were fully justified in ordering the destruction of the moth-eaten dodo skin, so long as they had no reason for doubting that other and better specimens were procurable; but now no labour and pains is considered too great in studying the most imperfect fragment of the bird.

And here I may perhaps be permitted to say a word in defence of what has been treated as an absurd practice on the part of palæontologists—that of giving names to small fragments of organisms. It must be admitted that when subsequent investigation proves that distinct generic and specific names have been given to the root, the stem, the outer and the inner bark, the pith, the foliage, and the fruit of the same plant, the absurdity does seem striking. But it is impossible to defer giving a name to a fossil until all doubts about its structure and affinities have been completely settled by the finding of exceptionally perfect specimens. Nevertheless, it ought certainly to be insisted on that names should be given to very fragmentary fossils only by a competent naturalist, and that he must accept the responsibility of his act. A single tooth of a mammal may afford good grounds for the establishment of a genus and species, while it might be utter folly to treat the tooth of a shark in the same manner.

The remains of many extinct forms are in such a peculiarly mineralized condition as to require special skill and training for their proper interpretation. Skeletal elements which were originally siliceous are now represented by pseudomorphs in calcite, and *vice versâ*. Characteristic structures in bones, shells, or wood may be wholly obliterated, and mineral structures of a strangely deceptive kind may be developed in their place. The curious story of *Eozoon canadense* and its supposed allies is surely a sufficient justification for the existence of palæontologists—that is, of specialists trained equally in the interpretation of biological and petrological structures. Dr. Sorby has shown that whole families of Mollusca may disappear from a fauna because of the unstable condition of the calcic carbonate which composes their shells, and his conclusions have been confirmed by Mr. Kendall. Prof. Sollas has similarly shown that the absence of the por-

cellanous types of the Foraminifera from the Palæozoic rocks may be due, not to their non-existence when those rocks were formed, but to the fact of their shells being composed of the unstable aragonite.

Such facts as these must convince any unprejudiced person of the absolute necessity, to the naturalist who attempts to study extinct forms, of an acquaintance with the nature of the mineral changes which organic remains undergo. In his interesting memoir upon those curious and enigmatical fossils, the Receptaculitidæ, Dr. Hinde has admirably shown the advantages of this combination of biological and petrographical study.

In this connection I cannot avoid alluding to a very prevalent and, as I cannot help thinking, very erroneous notion, that an intermingled zoological and palæontological collection, however inconvenient, would certainly be very instructive. To this view I offer the strongest protest, for I believe that the mistakes which would arise from the examination of such a collection would far outweigh any instruction to be derived from it.

I fail to see what useful lesson would be taught by swamping a collection of the lizards, snakes, tortoises, and crocodiles living at the present day with the vast slabs containing the relics of Reptilia which have existed in periods ranging from the Permian to the Pliocene. Nor is it apparent to me why the precious remains of *Archæopteryx* should be hidden away among a wilderness of bird-skins.

Any arrangement which could lead to the idea that even the richest collection of fossils is in any way commensurable with the assemblages of specimens that in our museums represent the existing fauna is very greatly to be deprecated. So numerous are the gaps among fossil faunas, owing to the fact that only animals with hard parts, and, as a rule, only those that lived in the sea, had any chance of preservation, that the finest palæontological collections are, and must always remain, extremely fragmentary. We have, in the past, fallen into so many and such grievous errors, by ignoring the imperfection of the geological record, that we may well hesitate before doing anything that would confirm this mischievous delusion.

On the other hand, it may be pointed out that our acquaintance with extinct forms of life has increased to such an extent in recent years that a biologist may well be pardoned for not realizing the vastness and importance of the problems involved in the study of fossils. It can only be a very inadequate idea of the value of palæontological evidence which leads fossils to be regarded (like the fauna and flora of a newly-discovered territory) as simply supplying a few missing links required to fill up gaps in a natural-history classification, or as the appropriate ballast for a Noah's Ark on a scale of national grandeur. Small as may be the whole bulk of a palæontological collection in the eye of the student of recent forms, its great and transcendent value depends on the fact that the objects composing it belong to the faunas and floras of periods widely separated from the present and from one another. The discovery of a new type of reptiles in the Trias is a very different matter from the detection of an equally remarkable form living in New Zealand. The latter may, it is true, be a singular survival of some old type; but the former is an actual landmark in the course of reptilian development; and by the study of the fossil we are actually brought much nearer to the solution of the problems connected with the history of that development than is possible by the study of any recent form.

In pointing out how vast has been the progress of our knowledge in recent years concerning the ancient life of the globe, I may remind you of the estimates made by Prof. Huxley when speaking from this chair a little more than a quarter of a century ago. He then characterized "the positive change in passing from the recent to the ancient animal world" as "singularly small"; and he regarded the extinct orders of animals as not amounting "on the most liberal estimate" to more than one-tenth of the whole number known. The evidence which has been accumulated during the last twenty-five years, however, has modified this estimate in a remarkable manner, as no one would be more ready to admit than the author of it himself.

There is no little difficulty in making a calculation of the proportion of living to extinct orders, owing to the discrepancies in the opinions of zoologists and comparative anatomists as to what are the characters which ought to be considered as of ordinal rank. For my present purpose I very gladly avail myself of the useful "Synopsis of the Animal Kingdom" prepared by Mr. E. T. Newton, which is "founded on the classification proposed by Prof. Huxley, with such modifications as are rendered necessary by recent discoveries."

We may, I think, take the whole number of living orders of animals generally accepted by zoologists at about 108. But in any comparison of these with fossil forms, it is only fair to exclude from our consideration such as possess no hard parts and stand little or no chance of being preserved in a fossil state. Few would be bold enough to doubt that such soft-bodied forms must have existed in the past, or that they probably bore about the same proportion to the forms with hard skeletons as in the existing fauna; even the boldest sceptic on this subject would, I should think, be convinced by such singular accidents as that of the finding of the impression of *Rhizostomites*, one of the Discophoræ, preserved in the soft calcareous mud of the Solenhofen Slate.

Now among the 108 living orders of animals, at least 36 are totally destitute of any hard parts capable of being preserved in a fossil state, and we have thus left 72 living orders with which our comparison of the extinct orders must be made.

What is the number of orders which must be created to receive extinct forms, is a question that has given rise to wide diversities of opinion in recent years. While few naturalists would consider 18 as an excessive estimate, there are others who would probably double that number.

Taking the lower estimate and comparing the 18 extinct orders with the 72 living ones which contain animals with hard parts, we find the proportion of extinct orders to be 20 per cent. of the whole number known at the present time.

But in comparisons of this kind, it must be remembered that there is an unconscious tendency among the students of recent forms of life to *under-estimate* the differences between extinct and living forms. If we take such groups as the *Graptolitidæ*, the *Monticuliporidæ*, and the *Stromatoporidæ*, of the nature of the polyps of which we can know nothing, we can only place them in existing orders on the ground of some very general analogies in the skeleton. How little this may be worth, recent zoological researches, like those of Prof. Moseley on the *Milleporidæ* and the *Stylasteridæ* have amply shown.

The students of existing forms of life have arranged their pigeon-holes; and into those pigeon-holes our unfortunate fossils are too often *made* to go. If there were no other objection to the wholesale commingling of recent and fossil types in a museum, there would be the valid and insuperable one arising from the fact that there are very considerable and important groups of fossils which cannot, without violence, be made to find any place in our accepted classification of existing animals—and perhaps never will.

If, however, we consider the modifications which have been brought about in our views concerning the relations of extinct to living forms by the important discoveries that have been made since 1862, we shall be impressed by the conviction that no comparison of the numbers of living and extinct orders can give any adequate idea of the important influence of palæontological studies upon biological thought. The discovery of transitorial forms, like the *Archæopteryx*, the toothed birds of America, and the reptiles with avian affinities, together with the working out of the rich faunas of the Rocky Mountains, of Pikermi, Quercy, and the Siwaliks, of the Pampean formations of South America, the Karoo beds of South Africa, and the caves of Australia, have already done much towards revolutionizing the ideas held twenty-five years ago by biologists concerning the significance and value of fossil forms. While the recognition of the less specialized precursors of such types as the horse and the elephant have perhaps produced most effect in removing objections to evolutionary doctrines, the light thrown by the study of fossil forms on the manner in which individual structures have arisen, as has been so well shown by Prof. Alexander Agassiz, in the case of the Echinodermata, opens up to us a wide and perhaps far more hopeful field of inquiry. We are, however, only at the beginning of the great task of utilizing the grand palæontological collections of mammals, of reptiles, of fishes, and of the various groups of the invertebrates, for explaining the significance and tracing the origin of the structures found in living types.

While maintaining that studies of this kind demand and justify the concentration of the labours of a special class of investigators, I feel sure that no one will misinterpret my meaning as to the qualifications required by the students of fossil forms. Far from suggesting that the palæontologist may be one destitute of a proper biological training, or that he may be satisfied with an equipment of knowledge which would be insufficient for a systematic zoologist or botanist, I would maintain that no one

has a right to take up the study and description of any fossil group until he has made a very careful and exhaustive study of its nearest living allies; but, in addition to this, he ought also to have made himself acquainted with the peculiar mineral changes which organic remains are liable to undergo. He will, moreover, be far more likely to interpret aright and to make the best use of the materials that come to his hand, if he have at least a general knowledge of what others working on similar materials belonging to other departments of the animal or vegetable world have been able to accomplish, and of the methods which they have followed. Such palæontologists, I insist, have as much right to recognition as any other class of biological specialists.

Still less should I wish it to be implied that I think systematic biologists can afford to be ignorant of the results of palæontological studies, in their own particular fields of labour. One of the most mischievous weeds that have accompanied the evolutionist in his incursions into various parts of the biological field is the preposterous "genealogical tree." We can scarcely turn over the leaves of a modern systematic work without finding it flourishing in full luxuriance. No sooner has the student of a particular group arranged his families, genera, and species, than he thinks it incumbent upon him to show their genetic relations. Very admirably has Prof. Alexander Agassiz pointed out the utter fatuity of such a proceeding. As Lyell used to say, in speaking of such proceedings, the imagination of the systematist, untrammelled by an acquaintance with the past history of the group, "revels with all the freedom characteristic of motion *in vacuo*." If for no other reason, zoologists and botanists ought to study fossil forms in order that, by encountering a few hard facts in the shape of fossils, they may be saved from these unprofitable flights of the imagination.

(*To be continued.*)

SCIENTIFIC SERIALS.

Rendiconti del Reale Istituto Lombardo, December 1887.—On the Tertiary formations near Cape La Mortola, in Liguria, North Italy, by Prof. T. Taramelli. The paper deals specially with the abrupt interruption which occurs in the prevailing Eocene and Secondary systems about this part of the Ligurian coast. This interruption is brought into connection with the great development in Liguria of the marine Pliocene formation, which in the Varo basin and near Ventimiglia stands at a present altitude of over 550 metres above the sea, but which does not occur at all further east in Istria and Friuli, where it is represented by thick alluvial deposits of vast extent.—On the neutralizers of tubercular virus, by Prof. Giuseppe Sormani. In continuation of his previous studies, the author here deals with twenty-one additional substances, or chemical reagents, making eighty altogether. According to their different action on Koch's Bacillus these are grouped in three categories: those that have no effect; those that only attenuate, and those that entirely destroy, the virus. As many as twenty-two, including camphorated chloral, the bromide of ethyl, and the nitrite of ethyl, are found to be effective.—Meteorological observations made at the Brera Observatory during the month of November 1887.

Rivista Scientifico-Industriale, January 15.—The crepuscular tints in connection with the hygrometric state of the atmosphere, by Prof. Costantino Rovelli. Constant observation shows that red and orange tints prevail in a dry, yellow and green in a moist, state of the atmosphere. This suggests a threefold division of the solar spectrum into (1) the region of warm rays transmitted by the lower atmospheric strata, and corresponding to a dry condition of the air; (2) the region of middle rays, yellow and green, more easily diffused and partly transmitted by the air in moist weather; (3) the region of cold rays diffused by an atmosphere abounding in aëriform vapour. The terrestrial dust suspended in the air, by condensing the aqueous vapour, as is now generally accepted, may also tend to produce those occasional after-glows of intense brightness, which have been so often observed after violent volcanic eruptions. The various character and intensity of the tints may all be thus explained by the theory of the eclectic transmission of the coloured rays by the corresponding states of the atmosphere, and partly also by the particles of dust held in suspense.—On the constitution of fogs and clouds, by Prof. F. Palagi. These phenomena are attributed to the presence of minute drops of water with diameter of 1/10 to 1/20 mm. at a temperature above zero. The recent observations made by the author on Mount Titano show that when the temperature falls below zero these globules are converted into minute hexagonal needles and flakes of the same form, the former about 1/20 mm. thick, and from two to ten times longer, the latter from 1/10 to 1/4 mm. in diameter. In their passage from the higher regions through the lower and less cold strata, but still below zero, these simple crystalline forms appear to be transformed by the process of condensation and agglomeration into the stars and flakes of ordinary snow. But when the temperature rises above zero they are again changed to the minute liquid drops of clouds, fog, and rain according to the varying degrees of altitude and temperature.

Bulletin de l'Académie des Sciences de St. Pétersbourg, vol. xxxii. No. 1.—On the effects of the earthquake of February 23, 1887, at the Observatory of Pavlovsk, by Dr. Wild (in German). The effects of the catastrophe having been observed at the Observatories of England, France, Italy, Germany, and Austria, in trepidations of the magnetic instruments, it was interesting to see whether the earthquake was felt as far as St. Petersburg. The results indicate that it was not.—On the genus Hemiculter and a new species of Hemiculterella, by N. Warpachowski (in German).—Russian words used in the Sagai dialect, and their phonetic modifications, by N. Katunoff; and lists of Sagai names of rivers, villages, and tribes, by the same. This little dictionary is highly spoken of by M. Radlof.—Studies, by O. Backlund, about the Pulkowa catalogue of stars, "Positions moyennes de 3542 étoiles," published in 1886 (in German). A detailed comparison of the Pulkowa catalogue with the measurements by Herr Romberg at Pulkowa, as also with the catalogues of Becker, Respighi, and Boss.—Hydrological researches, by Dr. Carl Schmidt.—The temperature-maxima before midday in tropical seas, according to the observations of the corvette *Vityaz*, by M. Rykatcheff (in German). They show the existence of two separate maxima, one of which sets in half an hour before midday and the other half an hour later. More extensive observations are needed.—On the synthesis of albumen in chlorophyll-bearing plants, by Chrapowitzki (in German). The chlorophyll spots must be considered as places where synthesis of both carbohydrates and albumen is going on.—New additions to the Asiatic Museum, by C. Salemann. Summaries of two Persian and three Kagatai manuscripts brought in by M. Pantusoff from the Semiryetchensk province.

THE *Izvestia* of the Russian Geographical Society (1887, iv.), contains most valuable papers and maps. Dr. Junker contributes a report on his seven years' journeys in Equatorial Africa, and his paper is accompanied by a map, 53 miles to the inch, of the region extending for ten degrees on the north of the Equator, between the 22nd and 33rd degrees of longitude. Two papers, by M. Potanin, contain a summary of the information gathered from the natives as to Eastern Tibet (the regions of Amdo and Kam), and the region of Central Mongolia situated between the Nan-shan, the Khangai, Hami, and the Utai-shan. Both papers are accompanied by maps, on a scale of 100 miles to an inch, and the two maps complement one another, so as to give a very accurate idea of the upper Hoang-ho. Of the other papers, one by M. Krasnoff, on the manners of life of the Kirghizes in the Semiryetchensk province, will be welcome to ethnographers. The same number contains also a list of fifteen places in Laponia, the latitudes and longitudes of which have been measured in 1864 by Captain Ernefeld; and, in a separate appendix, tables, by Prof. Sharnhorst, for the calculation of heights from barometrical observations. It is self-evident, although it is too often lost of sight, that the calculation of heights upon observations of the barometer, when it is made by means of logarithms, means a much greater accuracy of results than anything that can be obtained from a few observations of atmospheric pressure during a journey, and that some plainer tables would give the results with an accuracy quite sufficient for the accuracy of the data themselves. M. Sharnhorst's tables are an improvement upon those formerly in use, and ought to be introduced into every manual for travellers, instead of the usual logarithmical tables.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 26.—"Report on Hygrometric Methods. First Part, including the Saturation Method and the Chemical Method, and Dew-point Instruments." By W. N.

Shaw, M.A. Communicated by R. H. Scott, F.R.S., Secretary to the Meteorological Council.

With the exception of certain "absolute hygrometers," the behaviour of which has not yet been sufficiently tested, the determination of the pressure of water-vapour in the air is indirect, and requires a formula of reduction. The formulæ in use are based upon assumptions which are at present not so completely verified by experiment that any hygrometric method can be relied upon to give measures of the pressure of aqueous vapour trustworthy to within 0·1 mm. of mercury. The authority for these statements is given in detail in an account of the hygrometric work done since 1830, appended to the report as Note A.

In the report, the chemical hygrometric method is provisionally regarded as a standard.

The assumptions upon which the formula of the method is based are (1) that it is possible to absorb the whole of the moisture from air by passing it over desiccating substances; and (2) that a numerical value can be assigned to *d*, the specific gravity of aqueous vapour referred to air at the same temperature and pressure. The first assumption is sufficiently nearly accurate for hygrometric observations. With regard to the second, Regnault's direct observations upon steam (free from air) and other evidence point to the value 0·622. The assumption can, moreover, be tested, by applying the chemical method to air saturated at a known temperature, assuming the value 0·622 for *d*, and comparing the results with the table of saturation pressures *in vacuo*. This, however, *assumes Dalton's law to be strictly accurate*, an open question upon which opinion is reserved until further experimental investigation is concluded. Regnault found that the value 0·622 gave results for saturated air which were less than the tabulated pressures, the errors being always of the same sign, but so small in amount that he neglected them in his subsequent work.

The ultimate object of the experiments described in the report was to examine the behaviour of dew-point instruments in air of known state, and for this purpose air was saturated at a known temperature and drawn by an aspirator through vessels in which the dew-point instruments could be placed when required, and subsequently through drying tubes of special pattern. The vapour-pressure was thus obtained at the two extremities of the train of apparatus and the results compared.

The following questions are raised and discussed:—

(1) Were the drying tubes used as efficient as Regnault's? (2) Does the pressure of vapour in the air become changed by passing through the apparatus designed to contain the dew-point instruments, or by the mere presence of those instruments themselves? (3) Do the results of the chemical method agree with the tabulated vapour-pressures *in vacuo* when the air is more or less heated after being saturated? (4) Can the observed differences between the results be obviated by assuming a value for *d* (other than 0·622), which is compatible with values obtained by other methods? (5) Can any reason be assigned for the differences observed by Regnault in the case of saturated air?

(1) The answer to the first question is given in an account of a series of twelve experiments practically repeating Regnault's observations with saturated air. The tabulated results show divergences in the same direction and of the same order of magnitude as those in Regnault's paper. Some incidental points are also discussed—namely, the comparative efficiency of phosphoric anhydride, sulphuric acid, and calcium chloride, and the effect of india-rubber and glass connections between drying tubes. It is shown that the sulphuric acid and phosphoric anhydride tubes are efficient, that as a rule one tube is all that is strictly necessary, but that two should be used to provide for the case of exhaustion of the first tube or too rapid flow of air, and further, that the glass-and-mercury connections between the tubes employed in the second series of experiments cannot be regarded as producing any effect.

(2 and 3) The answers to the second and third questions are furnished by the results of eighty-two experiments with the chemical method upon air saturated at known temperatures by a specially designed "saturater" in a water-bath. The temperatures of saturation lay between 1° C. and 21° C., and, with one exception, were below the temperature of the surrounding air. Each experiment involves upwards of thirty readings of weight, pressure, and temperature. The temperature readings are corrected by means of a special comparison at Kew. Of the eighty-two observations thirty-two are retained as being free from any known disturbing causes, and from them it appears that, with *d* equal to 0·622, the pressure deduced by the chemical method is on the average greater by 0·03 mm. than that given in Regnault's table of vacuum pressures, as recalculated in Landolt and Börnstein's tables. This difference is very small compared with the discrepancies from Dalton's Law observed by Regnault in the case of water vapour.

(4) With regard to the fourth question; if the observations be employed to determine the value which must be substituted for *d*, the value obtained is 0·6245, which agrees very closely with 0·6240, the mean value for the same range of temperature deduced from Clausius's calculations based on thermo-dynamical reasoning. The value 0·622 is probably correct if the air is not nearly saturated; in that case the measure of the pressure of vapour in the air is 2/622 greater than it would be if the same air were reduced in temperature (at constant pressure) until it was saturated.

(5) The one observation of the second series with saturated air gives a result 0·18 mm. smaller than the tabulated pressure, and thus with the twelve experiments of the first series confirms the results of Regnault's observations. To account for this it is suggested that air which is very nearly or quite saturated would deposit some of its moisture on the glass tubes used to conduct it from one vessel to another. This behaviour of nearly saturated air has been already noticed, and it is confirmed by the observations on dew-point instruments, and moreover, by experiments, directly designed for the purpose, quoted in a note.

Details are given of observations with Regnault's hygrometer and Dines's hygrometer when exposed in glass vessels between the saturater and the drying tubes. The two instruments are separately discussed. With Regnault's instrument, after some practice, two different observers obtained practically identical results. In ordinary observations, the observed temperatures of the dew-point were below the temperature of saturation, but seldom by more than 0°·1 C. A considerable amount of uncertainty was shown to be attached to the readings, and by very close inspection readings of the dew-point were obtained above the temperature of saturation, in one case by as much as 0°·7 C.

From the experiments with Dines's hygrometer, it appears that the instrument is likely to give very easy determinations of the dew-point that are within small limits of error; but that, if it be observed with the closest attention, the result will be considerably too high in consequence of the formation of a dew deposit at a temperature above the dew-point, and it may possibly be erroneous in consequence of variations in temperature of the different parts of the box containing the thermometer.

An account is given of Alluard's modification of Regnault's hygrometer, and of Bogen's hygrometer.

A second note, B, is appended to the report, showing the tables used in various countries for the reduction of wet and dry bulb observations.

Chemical Society, February 2.—Mr. W. Crookes, F.R.S., in the chair.—Profs. Geuther, Ladenburg, Landolt, Nilson, Van't Hoff, Wislicenus and M. Lecoq de Boisbaudran were elected foreign members of the Society.—The following lecture was delivered:—The range of molecular forces, by Prof. A. W. Rücker, F.R.S. In discussing the range of molecular forces it is convenient to adhere to the language of the theory of action at a distance, though with full expectation that it will ultimately be replaced by another, such as the vortex-atom theory of Sir W. Thomson, or the granular theory of Prof. Osborne Reynolds, which involves only action in proximity. If we do this, however, it must be admitted that the law of molecular action may be very complicated. It may be granted that we naturally look for simplicity in our fundamental assumptions, but it is certain that we have *a priori* no more right to expect simplicity in the results of the action of a medium than simplicity in its constitution, and that the two are not necessarily obtained together. The largest values of the magnitude of the radius of molecular action which have been published have been deduced from observations on the condensation of gases and vapours on the surfaces of solids. Estimates on this basis made by Müller-Erzbach (*Exner's Rep.*, 1885, xxi. 409) and Kayser (*Wied. Ann.*, 1881, xiv. 450) have ranged between 1500 and 3000 micromillimetres[1] ($\mu\mu$). Such observations are open to many

[1] The micromillimetre is the millionth of a millimetre.

objections. Bunsen (*Wied.*, 1885, xxiv. 335) has shown that CO_2 will not condense on glass unless a film of water be previously formed. Warburg and Ihmori (*Wied.*, 1886, xxvii. 481, and *Wied.*, 1887, xxxi. 1006) adduce reasons for believing that the water film is largely due to uncombined or loosely combined alkalies on the surface. On clean unvarnished metals, washed glass and quartz, the thickness of the water film which can be removed by dry air without heating does not exceed 12 $\mu\mu$. A striking exception is agate, on which films 1640 $\mu\mu$ thick are stated to have been formed. As this substance, however, is composed of alternate layers of quartz and a porous impure opal, the basis for an accurate calculation does not exist. On the whole, it seems that no definite conclusions as to the magnitude of the radius of molecular action (ρ) can at present be drawn from these experiments. Quincke (*Pogg. Ann.*, 1869, cxxxvii. 402), as is well known, by measuring the capillary elevation of liquids between glass plates coated with thin wedge-shaped films, found $\rho = 50\,\mu\mu$. Plateau ("Statique des Liquides," 1873, i. 210) showed that the surface-tension of a soap-bubble, which thinned until its thickness was 118 $\mu\mu$, was unaltered. He concluded that $\rho < 59\,\mu\mu$. Maxwell ("Ency. Brit.," 9th ed., Art. "Capillary Action"), however, though by a confessedly imperfect theory, shows that the surface-tension may not change until the thickness of the film $= \rho$. Hence Plateau's result may mean only that $\rho < 118\,\mu\mu$. Reinold and Rücker (Phil. Trans., clxxvii. Part ii. 1886, 627) have proved that the surface-tension does not alter by 0·5 per cent., when the film is so thin as to show the black of the first order of Newton's colours. This appears at first sight at variance with Quincke's result, but their observations are really in remarkable accord with his. The black and coloured parts of a film are separated by a sharp line, which proves a discontinuity in the thickness (Proc. Roy. Soc., 1887, No. 182, 340). The colours, which correspond to certain thicknesses, which may be called the unstable range of thickness, are always missing. The black part of the film has been proved by Reinold and Rücker (Phil. Trans., Part ii. 1883, 645) to be of a uniform thickness, which differs but little from 12 $\mu\mu$. Sir William Thomson (Proc. Royal Institution) and these observers independently arrived at the conclusion that these curious phenomena are due to the fact that the surface-tension diminishes to a minimum, and then increases again when the thickness is somewhat $> 12\,\mu\mu$. The colours of the film prove that the upper limit of the range of unstable thickness is between 96 and 45 $\mu\mu$. Quincke's result indicates that it lies between 100 $\mu\mu$ and 50 $\mu\mu$, according as we adopt Plateau's or Maxwell's views. These calculations are therefore in complete accord. Quincke's result is not an isolated fact, but is supported by observations on soap films. The statement that 50 $\mu\mu$ and the radius of molecular action are of the same order of magnitude may now perhaps rank as an ascertained fact. Another method of investigating the radius of molecular action is based on the phenomena of electrolytic polarization, by observing the change in the difference of potential between a metal and a liquid in which it is immersed, when a gas or metal is deposited on it by electrolysis. In the former case we do not know the density of the gas, in the latter Oberbeck (*Wied.*, 1887, xxxi. 337) concludes that a plate of platinum is completely polarized by a film of another metal of from 3 to 1 $\mu\mu$ in thickness. The method of experiment is, however, open to objections, which are indicated by Oberbeck himself. Measurements of the thickness of the double electric layer of Helmholtz, which is closely related to the distance between two consecutive layers of molecules, have been made by Lippmann (*Compt. rend.*, 1882, xcv. 687), and by Oberbeck and Falck (*Wied.*, 1884, xxi. 157). The values they give vary between 1 and 0·02 $\mu\mu$. Wiener (*Wied.*, 1887, xxxi. 624) has studied the alteration in the phase of light reflected from very thin silver plates deposited on mica. He finds that the effect begins to alter when the thickness is reduced to 12 $\mu\mu$, and that it was possible to detect a silver film the thickness of which did not exceed 0·2 $\mu\mu$. The diameter of a molecule is a conventional term for the mean distance of the centres of two molecules during an encounter. It may therefore be different in the liquid and gaseous states. Sir William Thomson ("Natural Philosophy," Thomson and Tait, Part ii. 295, 1883), as the result of his celebrated discussion of this point, concludes that the mean distances between the centres of molecules in liquids (supposed arranged uniformly) is between 0·07 and 0·02 $\mu\mu$, and that the latter quantity is an inferior limit to the diameter of a gaseous molecule. The diameters of molecules (d) may be calculated if we know the mean free path (L), and the so-called condensation coefficient (v), which is the volume of the molecules contained in a unit volume of the gas. Loschmidt (*Sitzungsber. Wien. Akad. Math. Classe*, lii. abt. 2) and O. Meyer ("Die Kinetische Theorie der Gase," 225, 1887) have calculated d on the assumption that the molecules in a liquid practically fill the whole space it occupies. Exner (*Rep. der Physik*, xxi. 226, 1885), using a formula given by Clausius, $v = (K - 1)/(K + 2)$, where K is the specific inductive capacity, and can be replaced by $v = (n^2 - 1)(n^2 + 2)$, where n is the refractive index, finds values of d about five times smaller. Three independent methods of calculating the diameter of a gaseous hydrogen molecule lead to results between 0·14 and 0·11 $\mu\mu$. The most reliable conclusions which have been reached as to molecular magnitudes may be summed up in the following table, which is reproduced from a diagram exhibited during the lecture.

$\mu\mu$.		
118	Superior limit to ρ	Plateau (Maxwell)
96–45	Range of unstable thickness begins	Reinold and Rücker
59	Superior limit to ρ	Plateau
50	Magnitude of ρ	Quincke
12	Range of unstable thickness ends	Reinold and Rücker
12	Action of silver plate on phase of reflected light alters ...	Wiener
10·5	Thickness of permanent water film on glass at 23° C. ...	Bunsen
4·3	Mean distance between centres of molecules in gases at 760 mm. and 0° C.	O. Meyer
3–1	Thickness of metal films which polarize platinum	Oberbeck
1–0·02	Thickness of electric double layer	Lippmann and Oberbeck
0·2	Smallest appreciable thickness of silver film	Wiener
0·14–0·11	Diameter of gaseous hydrogen molecule	Exner O. Meyer Van der Waals
0·07–0·02	Mean distance between centres of liquid molecules	W. Thomson
0·02	Inferior limit to diameter of gaseous molecule	W. Thomson

—The following papers were read:—A new method of obtaining monohydrazides of α-diketones, by Prof. F. R. Japp, F.R.S., and Dr. F. Klingemann. The authors have prepared von Pechmann's monohydrazide of diacetyl by the action of diazobenzene chloride on sodium methacetate.—The formation of dihydrazides of α-diketones, by the same.—The action of phenylhydrazine on anhydracetophenonebenzil, by Prof. F. R. Japp, F.R.S., and Mr. G. N. Huntly.—The supposed identity of rutin and quercitrin, by Dr. E. Schunck, F.R.S. A comparative examination of rutin obtained from the leaves of *Polygonum fagopyrum* and of quercitrin shows that, though they are extremely similar, yet they differ in composition and in some of their properties. Rutin has the composition $C_{42}H_{50}O_{25}$, and yields, on hydrolysis, one molecule quercetin and three molecules isodulcite, whilst quercitrin $C_{36}H_{38}O_{20}$, as is known, yields, under like conditions, one molecule quercetin and two molecules isodulcite.—The composition of bird-lime, by Dr. E. Divers, F.R.S., and M. Kawakita. Japanese bird-lime prepared from *Ilex integra* contains, in addition to 6 per cent. of caoutchouc and minute quantities of oxalates, the ethereal salts of palmitic acid, and, in small quantity, of a semi-solid undetermined fatty acid. On hydrolysis these yield *ilicylic alcohol*, $C_{22}H_{38}O$, differing only slightly from Personne's ilicic alcohol, and *mochylic alcohol* $C_{26}H_{46}O$. A resinoid body, $C_{26}H_{44}O$, was also separated. When heated with palmitic acid, the two alcohols are converted into compounds just like purified bird-lime. The authors consider bird-lime to be closely allied to the *waxes* in chemical constitution.

ERRATA.—P. 335, line 15 (from top), *for* $3SOH_2SO_4$ *read* $3H_2SO_4$; line 19 (from top), *for* SO *read* SO_2.

Physical Society, January 28.—Prof. W. G. Adams, F.R.S., Vice-President, in the chair.—In opening the proceedings the Chairman referred to the great loss which the Society had sustained by the death of Dr. Balfour Stewart, their late President, and said that his loss would be deeply felt by the whole scientific world.—The following papers were read:—On the effect of magnetization on the thermo-electrical properties of bismuth, by Mr. Herbert Tomlinson.—On the influence of magnetism and temperature on the electrical resistance of bismuth and its alloys with lead and tin, by M. Ed. von Aubel.—On a water-dropping influence machine, by Prof. S. P. Thompson.—On the price of the factor of safety in lightning-rods, by the same. It is here shown, upon certain assumptions, that the safety against fusion varies as total cost $\times \dfrac{f s}{\rho d l^2 k}$ where f = temperature of fusion of material above atmosphere, s = specific thermal capacity, ρ = specific electric resistance, d = density, k = cost in pence per lb., and l = length of the conductor. If the total cost and length are supposed to be given, the factor of safety $= \dfrac{f s}{\rho d k}$. Of the common metals iron has the greatest factor of safety, being more than four times that of copper. Such being the case, the author thinks it desirable that the Report of the Lightning-Rod Conference be reconsidered.—On the optical demonstration of electrical stress, by Prof. A. W. Rücker, F.R.S., and Mr. C. V. Boys. A number of lecture experiments were shown illustrating that electrical stress exists in the dielectric separating two charged bodies. The bodies were placed in carbon bisulphide, between two crossed Nicols, and on electrifying them by means of a Holtz machine, light passed through the analyzer. Two concentric cylinders gave a black cross on the screen similar to those seen on interposing a plate of some uniaxial crystal, and a model illustrating a charged Leyden jar was shown.

February 11.—Annual General Meeting.—Dr. J. H. Gladstone, F.R.S., Vice-President, in the chair.—The Chairman read the Report of the Council for the past year, and expressed regret at the losses the Society had sustained by the deaths of Dr. Stewart (their late President), Prof. Kirchhoff, Mr. Coutts Trotter, and Prof. Humpidge. The Council regret that no increase of members has taken place during the past year, and hope that the advantages offered by the Society may be more fully appreciated in future. Obituary notices of Dr. B. Stewart, Mr. Coutts Trotter, and Prof. Humpidge were then read. The Treasurer's Report shows that the financial condition of the Society is very satisfactory. On the motion of Mr. Lant Carpenter, seconded by Mr. Inwards, the Reports were adopted.—The following gentlemen were elected members of Council for the present year:—President: Prof. A. W. Reinold, F.R.S. Vice-Presidents: Dr. E. Atkinson, Prof. W. E. Ayrton, F.R.S., Mr. Shelford Bidwell, F.R.S. and Prof. H. McLeod, F.R.S. Secretaries: Mr. Walter Baily, and Prof. J. Perry, F.R.S. Treasurer: Prof. A. W. Rücker, F.R.S. Demonstrator and Librarian: Mr. C. V. Boys. Other members of Council: Hon. R. Abercromby, R. H. M. Bosanquet, M.A., W. H. Coffin, Conrad W. Cooke, Prof. F. Fuller, W. N. Shaw, A. Stroh, Prof. S. P. Thompson, H. Tomlinson, G. M. Whipple. On taking the chair the new President expressed his sincere thanks for the great honour the Society had conferred upon him. Prof. Fuller proposed a vote of thanks to the Lords of Committee of Council on Education for the use of the rooms and apparatus of the Normal School of Science, which was seconded by Mr. Shaw, and passed unanimously. A cordial vote of thanks to the Council and officers of the past year, moved by Dr. Blaikley, and seconded by Prof. Ramsay, was duly acknowledged by the President. A similar vote, proposed by Mr. Bosanquet, and seconded by Mr. Haddon, to the auditors for the past year, was passed unanimously.—The meeting was then resolved into an ordinary science meeting, at which the following papers were read:—On the limit of refraction in relation to temperature and chemical composition, by Mr. T. Pelham Dale.—Note on the use of the term "resistance" in the description of physical phenomena, by Mr. R. H. M. Bosanquet.

PARIS.

Academy of Sciences, February 13.—M. Janssen in the chair.—On an ancient process for rendering gems and vitrifications phosphorescent, by M. Berthelot. The treatise in which this process is described occurs in the collection of Greek alchemists transcribed in certain manuscripts of the thirteenth and fifteenth centuries (Bibliothèque Nationale, Nos. 2325 and 2327). It contains a series of purely technical receipts analogous to those of the Leyden papyrus, some apparently of great antiquity, explaining certain methods of "colouring artificial precious stones, emeralds, carbuncles, hyacinths, according to the book taken from the shrine of the Temple." Several Egyptian alchemists are mentioned, such as Agathodemon, the pseudo-Moses, Ostanes, and Democritus, and the text leaves no doubt as to the ancient practice of rendering certain gems phosphorescent in the dark by means of surface colouring prepared from substances which are still known to possess such properties.—On the properties of a new hydraulic machine intended for irrigation purposes, by M. A. de Caligny. For this apparatus, which has been for some time in use both at Aubois and on the canal between Mons and La Louvière, the author claims the advantages of great simplicity of structure as well as economy on the first outlay. It may also be utilized for replenishing cattle-troughs, and other secondary uses, at a minimum cost. It was awarded a gold medal at the Antwerp Universal Exhibition.—On the part played by the absorbing power of the soil in the formation of the natural carbonates of soda, by M. Paul de Mondesir. The paper deals with Berthollet's well-known theory regarding the formation of Egyptian natron, and shows that Berthollet's explanation is so far true that the marine salt really furnishes the soda, and carbonate of lime, the carbonic acid. But the reaction is neither direct nor continuous, and is produced in two distinct phases. In the first, the soil reacts on the marine salt, transforming it into chloride of calcium while yielding lime and absorbing soda. In the second, which can set in only after removal of the chloride of calcium, the bicarbonate of lime and the carbonic acid extract the soda from the ground, replacing it with lime. Berthollet's theory is thus left fundamentally intact, but so modified as to become universally applicable. In fact, the carbonate of soda is produced in all permeable calcareous soils in proportion to the quantity of marine salt contained in them.—Observations of the new planet 272, discovered on February 4, at the Observatory of Nice, by M. Charlois. The observations, including right ascension, polar distance, and the positions of comparison stars, extend over the period from February 4 to February 9. When discovered the planet was of 13·5 magnitude.—New observations on the variability of Saturn's rings, by M. E. L. Trouvelot. It is pointed out that the observations made during the last few years by Perkins, Offord, Davis, Stanley Williams, Stroobant, and others, all tend definitely to establish the proposition announced by the author in 1884, that, so far from being stable, the rings of Saturn are on the contrary essentially variable, and subject to constant fluctuations. The same truth is confirmed by the author's own observations made in 1886 and 1887 at the Observatory of Meudon, and here communicated to the Academy.—Theorems on Campbell's algebraic equations and quadratic functions, by Father Aug. Poulain. Newton, or rather Campbell, formulated a very simple rule for determining the existence of the imaginary roots in algebraic equations. The author here proposes a few theorems, by means of which the application of this law may be extended and the accompanying calculations much simplified.—On chemical equilibria, by M. P. Duhem. In a recent note M. H. Le Chatelier announced that the numerical laws of chemical equilibrium, as deduced from the two principles of thermodynamics, may be expressed in a simple way by means of M. Massieu's characteristic function H′. Here it is shown that this law may be thus formulated: The variation imposed on M. Massieu's function H′ by a virtual isothermic modification of the system is equivalent to zero. It is further pointed out that the results obtained by M. Le Chatelier are practically identical with those arrived at by the author during a series of investigations spread over several years.—On the mineralizing action of the alkaline sulphides: reproduction of chrysoberyl, by MM. P. Hautefeuille and A. Perrey. During a protracted series of researches on the mineralizing action of the sulphides, the authors have succeeded in obtaining the crystallization of glucine, the separation of alumina and glucine, or inversely the reproduction of the aluminate of glucine, a combination which occurs in nature, and which is known by the name of cymophane (chrysoberyl). A simple process is described by means of which from a combination of glucine and alumina extracted from the emerald the

glucine may be obtained with a loss of not more than 10 per cent., and in such a state of purity that its equivalent has been found equal to 12·58.—Influence of various diets on the interchange of the gases in respiration, by MM. Hanriot and Ch. Richet. Continuing their researches on the respiratory function, the authors find that respiration increases with the increase of food, but only when this consists of the hydrates of carbon; that the interchange of the gases is but slightly affected by a nitrogenous and fatty diet; that feculent substances increase the absorption of oxygen and especially the production of CO_2; that the centesimal proportions of the absorbed oxygen or of the generated carbonic acid varies little during muscular repose; that the proportion of absorbed oxygen averages about 4·2 per cent., and of generated CO_2 about 3·4 per cent. The subject is illustrated by a diagram showing by a graphic process the influence of a nitrogenous and feculent diet on the respiratory functions generally.—Discovery of a worked flint and a mammoth's tusk at Vitry-en Artois, by M. Ladrière. The position in which these remains were found seems to confirm the author's view that towards the close of the early Quaternary epoch (Mousterian age) *Elephas primigenius* and other large mammals, as well as man, were already spread over the west of Europe.

BERLIN.

Physical Society, January 20.—Prof. von Helmholtz, President, in the chair.—Prof. Oettingen spoke on the interference of electrical vibrations which is produced by the electrical oscillations discovered by Feddersen, during the spark discharge. The discharge oscillations of two Leyden batteries, differing in frequency and amplitude, were allowed to produce an interference in the path of a third spark, and this led to a constant succession of alternatingly increased and diminished intensities of this spark. The phenomenon was analyzed by means of a rotating mirror, which resolved it into its several phases, and the events taking place in each spark were recorded by instantaneous photography. The speaker exhibited a large number of these photographs, both as negatives and as positive reproductions, and explained them fully. In these experiments, as in those described at the previous meeting of the Society on the explosion of an electrolytic mixture of oxygen and hydrogen, Prof. Oettingen had succeeded in obtaining accurate results only when he had replaced the concave rotating mirror by a plane one, whose action he then thoroughly discussed.—Prof. Börnstein exhibited a preparation which he had recently obtained quite by chance, during one of his lectures. When lecturing on the diffusion of liquids, he was in the habit of using a Traube artificial cell. On placing a blue crystal of sulphate of copper in a solution of soluble glass, a precipitate is formed as a film on the surface of the salt, when it comes in contact with the soluble glass. The water from the solution then diffuses through the film, dissolving the salt and stretching the film until it is ruptured at some one point. When this occurs the solution of the sulphate of copper comes again into contact with the soluble glass, a new film is formed at the surface of contact, closing up the aperture, and the diffusion begins again. The film thus grows continually in a tubular form, until it finally permeates the whole solution. When recently repeating this lecture experiment, the speaker noticed that the film did not grow in the usual tubular way, but took the form of flattened parallel membranes which advanced through the solution at right angles to their length. He was at present unable to offer any explanation of this latter phenomenon. —Dr. Budde had recently submitted Clausius's fundamental law of electro-dynamics to a recalculation, while taking into account a large series of special conditions; among these he allowed for the motion of translation of the earth, and found that it had no influence on the validity of the law. At that time he had not calculated the influence of the earth's rotation; he had however, since then, repeated his former work, and gave an account of the results of his calculation, which showed that the rotatory motion of the earth had also no influence on the law. The same speaker finally drew attention to an error which occurs in all text-books, in connection with the determination of the potential of a system of points, and showed how illogical is the usual definition and of deducing of potential energy. Prof. von Helmholtz then directed attention to the fact that he was in the habit of determining potential energy in a different way, and that its derivation from a system of points is fraught with great difficulties.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Contributions to the Palæontology of Brazil: C. A. White (Washington).—Die Entstehung der Arten, 1 Thiel: Dr. G. H. T. Eimer (Fischer, Jena).—Key to Todhunter's Differential Calculus: H. St. J. Hunter (Macmillan).—Annals of the Astronomical Observatory of Harvard College, vol. xiii. Part 2, Zone Observations made with the Transit Wedge Photometer (Wilson, Cambridge, Mass.).—Calendar and General Directory of the Department of Science and Art for the year 1888 (Eyre and Spottiswoode).—Electrical Instrument Making for Amateurs: S. R. Bottone (Whittaker).—Practical Education: C. G. Leland (Whittaker).—Volapük, or Universal Language: A. Kirchhoff (Sonnenschein).—Geology: Chemical, Physical, and Stratigraphical, vol. ii.: J. Prestwich (Clarendon Press).—Observations made during 1883 at the U.S. Naval Observatory (Washington).—Die Prähistorischen Denkmäler der Provinz Westpreussen und der Angrenzenden Gebiete: Dr. A. Lissauer (Williams and Norgate).—The Shell Collector's Hand-book for the Field: J. W. Williams (Roper and Drowley).—My Telescope: A Quekett Club Man (Roper and Drowley).—Through the Yang-tse Gorges: A. J. Little (Low).—Report on the Administration of the Meteorological Department of the Government of India in 1886-87.—Indian Meteorological Memoirs, vol. iii. Part 2 (Calcutta).—A Manual of the Geology of India; Part 4, Mineralogy: F. R. Mallet (Trübner).—Bulletin of the U.S. Geological Survey, No. 39 (Washington).—The Law of the Universe: G. W. Cleverley (Brown, Hull).—Quarterly Journal of the Geological Society, vol. 44, Part 1, No 173 (Longmans).—Proceedings of the Linnean Society of New South Wales, 2nd series, vol. ii. Part 3 (Sydney).—List of Contributors to ditto, 1st series (Sydney).—Quarterly Journal of Microscopical Science, February (Churchill).—Journal of the Royal Microscopical Society, February (Williams and Norgate).

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